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ABSTRACT

This report represents the completion of an extensive evaluation of pre-employment programs as potential strategies for moving disadvantaged youth into private sector jobs. The report inspects long-run impacts on participants in five "70001 Pre-employment Programs" by analyzing data from followup interviews of participant and comparison youth between 24 and 40 months after intake into the program. (The 70001 programs couple modest job-readiness and educational training with job placement; the opportunity to take GED classes is also provided.) Results show that although 70001 had a positive effect on youth employability in the short run, there is no evidence of permanent or long-run effects on employability, as measured by subsequent likelihood of employment, earnings, or type of job held at 24 to 40 months after intake into the program. Participation did, however, positively affect GED attainment. A discussion of methodology and supplementary tabular data about program participants are provided in the appendices. (CMG)

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Longer Term Impacts Of Pre-Employment Services On the Employment And Earnings of Disadvantaged Youth

A Project Of the Private Sector Initiatives Demonstration of Public/Private Ventures

May, 1983

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EXECUTIVE SUMMARY

This report represents the completion of an extensive evaluation of pre-employment programs as potential strategies for moving disadvantaged youth into private sector jobs. The report inspects long-run impacts on participants in five 70001 Pre-employment Programs, who enrolled in these programs between January 1979 and April 1980, by analyzing data from follow-up interviews of participant and comparison youth between 24 and 40 months after intake into the program.

The 70001 Program

70001 Ltd. is a national organization with over 50 locally operating programs, designed primarily to serve disadvantaged youth. The program couples modest job-readiness and educational training with job placement in order to help youth gain a hold in the labor market. Pre-employment training provided by the program includes but is not limited to such activities as how to fill out a job application, how to handle oneself in a job interview, shaping realistic work behavior, and how to interpet help-wanted ads. The average participant receives 32 hours of such training. Participants also receive career counseling, job placement, and the opportunity to take GED classes.

70001 participants range in age between 16 and 21. All are economically disadvantaged, and over 80% came from minority groups. Sixty percent are women. Practically all of the participants are high school dropouts.

Research Tasks

The research in this report focused on 975 youth in five cites. The participant group consists of the entire population of 535 youth who enrolled in the 70001 program between January 1979 and April 1980 in the five directly-operated sites of Atlanta, Boston, Richmond, San Antonio, and Tulsa. The comparison group is made up of 440 similar youth from the same cities, and for the most part drawn from lists of registrants at local job service offices. It is important to note that the study used a comparison group rather than control group methodology. Participant and comparison youth are very similar in terms of background characteristics, but they were not randomly assigned to each group.

Two previous reports on this sample -- one based on follow-up data averaging nine months since intake and the second based on follow-up data averaging fourteen months since intake -- found that 70001 participants had statistically significantly higher

earnings than comparison youth. The purpose of this study -- consisting of follow-up interviews taken between 24 and 40 months since intake -- was to determine if the early positive effects of the 70001 program persisted over time.

Key outcomes examined in this report include weekly earnings, probability of employment, and occupational attainment. These outcomes are studied both at the point-in-time of the 24 to 40-month follow-up in May and June of 1982 and for calendar year 1981. The outcomes in this third follow-up wave are also compared with the results from the two earlier follow-up waves in order to determine changes over time in program effects. Because all the interviews in this 24 to 40-month follow-up took place during a recession, and because the females in the sample are in an age group characterized by high rates of childbearing, special attention was paid in the report to separating out recessionary and childbearing effects from changes over time in program effects.

Results

Before discussing results, it is important to note that the 70001 programs that we chose for follow-up interviews (that is, the directly-operated ones) performed better on short term outcome measures (placement in jobs) than those sites not chosen for follow-up. This might suggest not only that one cannot generalize from these five sites, but also that analyses based on them might overstate the benefits to youth. On the other hand, one must be cautious at taking these results (from experiences in programs in 1979 and 1980) and generalizing to 70001 programs today since some modifications have undoubtedly been made to strengthen the benefits to youth.

The following findings are derived from the last wave of follow-up interviews on 70001:

- After doing various model specifications, we found no statistically significant evidence that the 70001 programs investigated here have impacts on the longer run employability of their participants. While positive effects of the program were found at "9" and "14" months after intake, by the 24 to 40-month post-intake period, these effects appeared to have decayed. The effects in the 24 to 40-month post-intake period are near zero for the total sample and for males, but negative and statistically significant for females. We found no evidence of the impact of 70001 on the occupational skill level of participants nor on the annual earnings of participants in calendar year 1981, that is, 21 to 33 months after program intake.

- The apparent decay of program effects in this 24 to 40-month period appears to result in large part from an improvement in the labor market position of the comparison group, in the face of a deep recession, at the same time that the participants are experiencing a slight deterioration of their position in the labor market. There is little convincing evidence that the recession was largely responsible for the decay of program effects. Differential childbearing by the participant and comparison groups cannot explain the negative program effects for females, although childbearing and responsibility for dependents has clearly been influential in the differential patterns of labor force behaviors of the male and female participants.

- In this longer term follow-up, 70001 sites investigated here were shown to have a positive impact on GED attainment.

- The program is relatively inexpensive when compared with other treatment modalities. However, it is one of the more expensive pre-employment programs.

- When using a "pay-back" analysis, the program is not cost-effective for females, as females can never pay back the cost of the program. The program is quite cost-effective for males as they pay back the cost of the program in just over one year, and is almost, but not quite cost-effective for the overall sample, as participants come close to but are never quite able to pay back the cost of the program. (Note that this reversal from the earlier report is due to the fact that early participant gains were offset by subsequent participant losses relative to the comparison group.)

In sum, these results suggest that our research has provided evidence that the 70001 programs investigated here have a positive effect in the short run, but no evidence was found to suggest that 70001 had longer run impacts on youth's employability.

Implications for Policy Makers

The research confirms that well-run pre-employment programs such as 70001 can:

- place disadvantaged, out-of-school youth in private sector jobs at a low cost per placement and without offering direct subsidies to employers (nor do employers take advantage of tax credits)

- attract same youth to job-readiness training activities without paying a stipend

- o increase youth earnings, at least temporarily, relative to a comparison group

- o work for youth under 18 as well as for older youth.

Moreover, it should be noted that the 70001 programs investigated here have a more enduring labor market impact than many other such pre-employment programs studied through the Department of Labor.

However, we have found no evidence that the 70001 programs investigated here have permanent or long run effects on the employability of youth, as measured by subsequent likelihood of employment, earnings or type of job held at 24 to 40 months after intake to the program. (Note, in this longer term follow-up, participation in 70001 did positively affect GED attainment.) Thus, while the program meets many of its objectives, such as placing youth in private sector jobs and increasing earnings in the short run, we find no evidence of the ability of these five 70001 programs to permanently affect the youths' longer-run employability. In short, the programs place youth in private sector jobs, allowing them to reap earnings gains, but eventually comparison group members will get jobs on their own and the advantage to participants disappears. Despite the fact that the 70001 programs meet many important objectives, one is left wondering whether pre-employment programs such as 70001 are good social investments. For while these programs are able to increase participants' earnings in the short run, they do not appear to be able to increase youths' earnings in the long run. Additional interventions might be required subsequent to pre-employment experience to sustain the earnings gains produced by the program. Especially for youth under 18 years of age, pre-employment programs like 70001 may be only the first step in a series of necessary interventions.

CHAPTER I

INTRODUCTION

Since 1978, Public/Private Ventures has been conducting a multi-site demonstration project for the Department of Labor's Office of Youth Programs. The project aimed at developing and assessing a variety of approaches to private sector participation in employment and training programs for 16 to 21 year-old, economically disadvantaged, out-of-school youth. Overwhelmingly these are poor, minority drop-outs who find getting work to be a "maddening, dead-end course." The Private Sector Demonstration encompassed six distinct interventions: pre-employment services, temporary work experience, targeted skills training, subsidized work experience, youth entrepreneurship and small business incentives. Final reports from each intervention have been completed.

P/PV's work on pre-employment programs focused on two such programs: 70001 Ltd. and Jobs for Youth, Inc. To date, two reports have been written which summarize the results from the follow-up interviews which were collected at "nine" and "fourteen" months post-intake (approximately three and eight months after the "termination time", which was artificially set at six months post-intake.)[1] While these two reports were to have been the completion of the investigation of the effects of pre-employment programs on disadvantaged youth, the results from the second report were such that it seemed appropriate to do an additional wave of follow-up interviews on one of these programs, 70001 Ltd. This report, therefore, analyzes these last wave follow-up data in order to inspect the longer term effects of a particular pre-employment program. This chapter sets the stage for the rest of the report by first recounting the earlier results (and the puzzles therein), discussing the basic research design used in this third wave of interviews, describing the 70001 model and then outlining the chapters to come.

Results from the Earlier Research

The earlier research[2,3] on 70001 and the JFY program in Boston indicated that these programs:

- place disadvantaged, out-of-school youth in private sector jobs at a low cost per placement and without offering direct subsidies to employers (nor do employers take advantage of tax credits).
- attract youth to job-readiness training activities without paying a stipend.
- increase youth's earnings, at least temporarily, in amounts sufficient to make the programs cost-effective.

- work for youth under 18 as well as for older youth.

While these general statements can be made about both programs, 70001 and JFY exhibited different patterns of program impacts over time.

- 70001 had a positive and statistically significant effect on the short-run earnings of youth, and the effect appreciated over time, at least through the "14" month post-intake period. "Nine" months after intake, 70001 youth were earning an average of $14.15 more per week than their matched comparison counterparts when other factors were held constant. At "14" months, the difference had grown to $24.40.

- JFY Boston had pronounced and statistically significant short-run effects on the earnings of its participants, but by month "14," the net earnings impact of JFY over its comparison group had decreased and was not statistically significant. "Nine" months after intake, JFY had a net impact of $28.08 on the weekly earnings of participants over their comparison cohort. By "14" months post-intake this impact was $12.02 and was not statistically significant relative to comparison group earnings.

The JFY effects were not puzzling. Often, in pre-employment type programs which are inexpensive and of short duration, program effects "decay" over time as members of the comparison group eventually find jobs and achieve parity with participants. It should be noted at the outset that the term "decay" as used in this report refers to a "lessening of an impact or effect over time." Decay does not necessarily imply that participants are doing worse over time, only that the gap between participants and comparisons is diminishing. However, 70001's increasing effects over time as well as the divergent patterns for the two programs were surprising.

We could not account for these surprising findings without further study. Several potential explanations for these results were assessed empirically and then ruled out: attrition in the follow-up sample, differences among the sites in which the programs were located, the presence of some atypically high earners, and the differences in the sex composition of the two programs.

Two potential explanations remained. The first related to the fundamental differences between the two programs. JFY emphasized rapid non-individualized placement, and moderate service levels, which could result in a rapid gain but an equally rapid decay. On the other hand, 70001 with its more individualized job placement system and more intensive services (and more post-placement services), might be expected to exhibit a delayed but more prolonged impact.

Yet, it would be incorrect to accept this explanation before ruling out an "artifactual" one that could also explain the differential patterns of program impact. There is no official time of termination from either of these programs, so an artificial termination date of six months after intake was chosen, after it was determined that participants, by and large, had no further contact with the program after this point. It is possible that this artificial termination date is too early for 70001 or too late for JFY. If this is the case, it is likely that there is a time lag problem, such that 70001's program impact would look exactly like JFY's (strong early gains with a rapid decay), if the time lag problem were taken into account.

This explanation could not be assessed without another wave of follow-up interviews. If 70001's impact has decayed at the next wave of interviews, then this would provide some empirical support for the explanation of the earlier surprising results that the pattern of effects is similar for the two programs, with the effects for 70001 lagged a number of months (perhaps due to the artificial termination date). If 70001's effects do not decay, or decay quite slowly, then we must revert to the explanation concerning differential program services.

Research Questions and Research Design

The basic question to be answered is: what are the longer run impacts of the 70001 program? Additionally, the follow-up data help distinguish between stepping stone and aging vat concepts of pre-employment programs. If the minimim wage jobs found for program participants are stepping stones to skilled careers, then the 24 to 40-month follow-up data should begin picking up some movement in the participant sample to higher skilled jobs. If the minimum wage jobs are helpful as aging vats for participant youth, then participants should have higher rates of employment and labor market participation than comparison youth.

To address these basic questions, an additional wave of follow-up interviews was completed for the five 70001 programs previously investigated. It was decided that additional interviewing was not necessary for the JFY participant and comparison samples, since JFY's effects had decayed by the last wave of interviewing.

After funds were received from the Department of Labor, all youths who were in the original 70001 participant or comparison samples were scheduled for interview in May and June of 1982. This was between 24 and 40 months after their initial entry into the program (or in the case of the comparison group, from the time they sought services). The Institute for Survey Research at Temple University conducted the follow-up interviews and obtained an impressive 86% response rate.

70001 Model

Before outlining the rest of the report, we will briefly describe the 70001 pre-employment program model. (The reader is referred to the two earlier reports on pre-employment services for a lengthier description of the programs.) The term "pre-employment services" has been applied to career counseling, job-readiness training, self-directed job search instruction, remedial education, "try before hire" agreements and a host of other service offerings. In this report, however, the operational definition is perhaps best conveyed by functionally synopsizing the 70001 programs investigated here, that is, the 70001 program as operated in five sites between January 1979 and April 1980. They couple modest job-readiness and educational training with job placement in order to help young people gain a hold in the labor market in such entry-level jobs as are available to unskilled workers. They provide youth with intensive but limited doses of career counseling, educational services, and pre-employment training, featuring such activities as 1) how to fill out a job application, 2) how to handle oneself in a job interview, 3) shaping realistic work behaviors, such as punctuality and proper dress, and 4) how to interpret help-wanted ads.

A distinguishing feature of 70001 is that no financial incentives are paid. Youth receive no stipend for participating in the training; employers receive no reimbursement for hiring a youth. It is worth noting that the term "pre-employment" program is somewhat of a misnomer for 70001 because close contact is continued with youth as well as employers after placement in private sector jobs.

70001 was begun in 1969 and within a decade burgeoned to over fifty local programs, bound together by a strong national office. Its dominant modus operandi is a network of affiliated programs, each administered by a local sponsoring agency and funded largely via CETA prime sponsors. The central office, consisting of approximately fifty staff, supports each local program by providing technical assistance, help in screening and hiring staff, centralized staff training, and promotional materials. The central office, located in Washington, D.C., is buoyed by an active board of directors. In addition, a national advisory business group and a national bipartisan Congressional advisory association furnish high visibility and close contact with business and legislative developments.

The prototypical 70001 delivery agency is by design not large. Most serve from 75 to 125 youth per year. The typical participant in the sample studied here was 17 or 18 years old at intake, black and economically disadvantaged. Practically all youth were high school drop-outs (99%) with one-fifth of these completing only the eighth grade or less. Not surprisingly, a large segment read at a level well below chronological age; 50% of the participants obtained less than a seventh grade equiva-

lency score on the Adult Basic Learning Examination (ABLE). In terms of previous work experience, three out of ten youths had never held a regular job. Of the youth citing previous jobs, practically all were secondary labor market positions, with a third of these paying below the minimum wage.

Sixty percent of the 70001 participants are women. 70001 staff suggest that female enrollees may be attracted to the program's more traditional educational offerings because the majority of these women were "forced" to leave school due to pregnancy and welcome the prospect of 70001's General Educational Development (GED) course. Recruiting patterns have also been suggested, with the rationale being that the peer grapevine, the most common source of youth referral, tends to promulgate along sexual lines. If early participants are mostly female, later ones will also be female, and vice versa.

The typical participant hears about the program from friends, but 70001 has made good use of media advertising, which accounts for one in four participants' awareness of 70001. First contact consists of explaining the program and inviting youth to a small group orientation. These range in duration across local sites from a half-day to a full week. Orientation aims at providing youth sufficient information about program requirements and activities so that each young person can make a knowledgeable decision about entering the formal phase of the program.

For those who proceed, each must successfully complete a mandated (16 hours minimum) classroom regimen that focuses on preparation for labor market entry. This component, dubbed PET (Pre-Employment Training), includes such content areas as identifying and responding to job openings, grooming and proper dress, workplace requirements such as punctuality and courtesy, filling out job applications, and the like. Full-time instructors apply a variety of techniques such as role play, video-tape feedback and field trips to businesses. From 50 to 80 percent of each class completes PET, the average participant received 32 hours of pre-employment training.

Graduation typically is followed by job placement, which occurs through an individualized process. A roster of employers, who have shown interest based on previous participation or earlier visits and mailings, is used in focusing the job search. Half of the youth who enter the 70001 program are successfully placed in unsubsidized jobs. As a means to job acquisition, emphasis is placed on job-readiness training, socialization, community values and especially education in the form of GED study. However, no relationship was evident between degree or type of service received and job placement.

While most job development is accomplished by local staff, some is done with the assistance of the central office, which solicits local leads and recommendations from national business

associations. Staff try to match a prospective employer with a given youth's interests and abilities. Personal contacts with firms are made by 70001 staff usually in the form of a phone call followed by an interview at the place of business. Although there is some variance in approach, staff tend to downplay a "hire a disadvantaged youth" appeal and accentuate the fact that the youth desires to work and has successfully completed an intensive job-readiness program.

Since most staff at 70001 implement all program functions, it is usually true that the staff member who develops the job is also the counselor who assigns the youth to that job. The 70001 program stresses a young person's retention of his/her original job placement. Staff at 70001 commonly accompany the candidate to interviews and use the experience in subsequent counseling sessions. The 70001 "job coordinator" continues post-placement contact with youth and employers in order to respond to problems.

Education is accentuated at 70001, although it is not compulsory in all sites. Standardized curricula are used at both the pre-GED and GED level, and youth who do go on to gain a GED are cited in 70001's national magazine. Often youth will spend mornings in the PET sequence and afternoons in formal education endeavors. For youth who have obtained jobs, evening classes are offered so that they can continue studying for their GED certificate. The educational component is so pronounced at 70001 that in several cities 70001 is perceived by youth and referring agencies as much as an alternative school as an employment program. Youth in the sample studied averaged ten hours of pre-placement education and the same amount after placement. However, very few attained GEDs during their program participation. (See Chapter V for evidence that they are significantly more likely to attain GEDs in the longer run.)

70001 also places emphasis on the youth socialization process, a distinguishing feature not often found in employment and training programs. Each local program is required to begin a SEVCA Chapter (70001 Career Association) which is a cross between a social club and a service organization. Its aim is to provide peer support for young persons, concomitantly increasing community and career awareness. Local SEVCA events might range from a neighborhood clean-up to a dance, travel to regional and national conferences where youth participate in seminars, workshops and competitions. It is difficult to calibrate SEVCA's contribution to youth employment, but it may offer considerable psycho-social support during a difficult time of career and personal development. Some staff have suggested that SEVCA offerings are more appealing to young women than to men, and this could contribute to 70001's high female representation.

Outline of the Report

Chapter II continues the discussion of research design issues. It presents the characteristics of the participant and

comparison groups, the effects of attrition and several issues related to modeling, such as how to control for unobserved differences between the participant and comparison group.

Chapter III presents the results from the analysis inspecting the effects of the program on labor market outcomes, including employment status, occupation, and earnings. We examine these outcomes both at the point-in-time of the 24 to 40-month post-intake follow-up interviews and for calendar year 1981. We also combine our 24 to 40-month follow-up data with two earlier waves of follow-up information in order to estimate changes over time in program effects.

Chapter IV inspects two potential explanations for the effects found in Chapter III. It investigates the possible effects of the recession on the labor market behavior of these disadvantaged youth and the potential effects of childbearing. Chapter V considers the effects of the program on other outcomes. In Chapter VI, we examine estimates of program costs and compare these costs to program benefits. Chapter VII reviews the major findings and makes policy recommendations.

Notes

1) A more complete discussion of this issue can be found in the final report on pre-employment services. See (3) below.

2) Public/Private Ventures, Second Interim Report: The Impact of Pre-employment Programs on Disadvantaged Youth: A Short Term Follow-up Study. Fall 1981. Philadelphia, Pennsylvania.

3) Public/Private Ventures, The Impact of Pre-employment Services on the Employment and Earnings of Disadvantaged Youth. Final Report, April 1982. Philadelphia, Pennsylvania.

CHAPTER II

PARTICIPANT AND COMPARISON SAMPLES

The original sample for this analysis consists of 535 participants and 440 comparison youth. The participants are the entire population of youth who enrolled in 70001 programs between January 1979 and April 1980 in the five directly-operated sites of Atlanta, Boston, Richmond, San Antonio, and Tulsa. For purposes of this study, enrollment was defined as the point of intake into the program, which usually represented a youth's second visit to the site.

It is important to note that the 70001 programs that were chosen for follow-up interviews (that is, directly-operated ones) performed better in short term outcome measures, such as placement in unsubsidized jobs, than those 70001 programs not chosen for follow-up (see the earlier report). This suggests that one must be cautious in generalizing to all 70001 programs, since analyses based on our follow-up sites may overstate the benefits to youth. On the other hand, one must remember that we are investigating the long run effects on youth who entered these five 70001 programs during 1979 and early 1980; modifications in the program since then could alter the long run benefits to youth.

It is also important to note that this study used a comparison group rather than a control group methodology. The youth were not randomly assigned to participant and comparison samples. Rather, efforts were made to collect a comparison group of youth who were as similar as possible to participant youth, both in terms of background characteristics and the date at which they applied for program assistance. The comparison sample, for the most part, was matched youth for youth with the participant sample according to city, sex, age, race/ethnicity and month of participant intake. Virtually all comparison youth were economically disadvantaged, unemployed, and high school dropouts. They were drawn almost entirely from among registrants at local job service offices. Where this procedure failed to yield enough youth, additional comparison youth were selected from lists in CETA prime sponsor "not enrolled" files and from dropout lists from schools and community-based organizations. (Note that this latter procedure could introduce a slight bias in favor of the program.) Table II-1 contrasts the participant and comparison samples on selected background characteristics at the baseline interview. Few statistically significant differences appear. (Tables for background characteristics of male and female participant and comparison samples are shown in Appendix Tables A-II-1 and A-II-2.)

In the "9"-month post-intake follow-up, 303 participants and 346 comparisons were interviewed for a completion rate of 66%. In the "14"-month post-intake follow-up, 246 participants and 264 comparisons were interviewed for a completion rate of 52%. It

TABLE II-1

Background Characteristics at Intake
Participant and Comparison Groups

	Participants n= 535	Comparisons n= 440
Average age	17.9	18.2*
Sex		
male	41%	40%
female	59%	60%
Ethnicity		
white	18%	14%
black	62%	63%
Hispanic	20%	23%
High school degree	1%	1%
Average reading score (grade level)	6.8	6.6
Average number of dependents	.34	.50*
Head of household	12%	13%
Family of one	11%	10%
Member of family	77%	77%
Receiving AFDC	20%	15%*
Ever held pre-program job	71%	75%
Average pre-program wage	$2.06	$2.30*
Atlanta	19%	23%
Boston	19%	15%
Richmond	21%	19%
San Antonio	25%	20%*
Tulsa	17%	22%

*Indicates difference between groups reaches statistical significance at the .05 level.

should be noted that in the "14"-month follow-up, an effort was made to track only those sample youth who had been interviewed in the "9"-month follow-up. However, in the 24 to 40-month follow-up, an effort was made to reach all 975 youth in the original baseline survey, regardless of whether they had been interviewed at an earlier stage. The Institute for Survey Research at Temple University was awarded the sub-contract to conduct these interviews. ISR reached 451 of 535 participants and 392 of 440 comparison youth, for an overall survey completion rate of 86%.[1]

Statistical tests were conducted to test for possible sample attrition bias. Table II-2 compares the baseline characteristics for attriters and those followed-up at 24 to 40 months. (Tables for males and females separately appear in the Appendix as Tables A-II-3 and A-II-4.) As is clear in the tables, attrition does not appear to affect the participant and comparison groups differentially.

Controlling for Selection Bias

As is clear in Table II-1, there are some differences between the participant and comparison samples in characteristics such as age and number of dependents. Differences between the comparison and participant groups cause no problem as long as all the differences between the groups are observable. The purpose of the multivariate analyses in the following chapter is to control for differences in observable characteristics such as age, race/ethnicity, number of dependents, and high school degree. However, in non-randomly chosen samples such as these, there is the possibility that both self-selection and program selection procedures could result in unobserved differences between the participant and comparison samples. One is only concerned about unobservable differences that would affect outcomes, such as motivation and self-initiative. Theoretically, the potential biases introduced by such unobserved differences could operate in either direction, that is, in favor of or against the program. More motivated youth may be attracted to 70001 or, on the other hand, youth with more self-initiative may feel they can do better on their own.

In an attempt to take into account such unobserved differences between the participant and comparison groups, we used a recently developed econometric procedure which controls for selection bias in the estimation of program effects. This procedure involves a two stage methodology which attempts to capture unobservable characteristics which affected selection into the program. In the first stage of this analysis, selection into the program is modeled based on various observable differences between sample individuals. From this model, conditional probabilities of participation based on observable characteristics are estimated for each individual. Differences between the predicted and actual participation status of each individual can then be attributed to unobservable individual characteristics.

TABLE II-2

Attrition in 24 to 40-Month Follow-up Sample

	Participants		Comparisons	
	Followed-up	Attriters	Followed-up	Attriters
	n=451	n=84	n=392	n=48
Average age	17.9**	17.9	18.2	18.4
Sex				
male	40%	46%	39%	46%
female	60%	54%	61%	54%
Ethnicity				
white	13%	24%	16%	22%
black	63%	57%	63%	63%
Hispanic	24%	19%	21%	15%
High school degree	1%	1%	1%	0%
Average reading score	6.8	6.7	6.6	7.1
Average number of dependents	.34**	.31	.48	.63
Head of household	12%	11%	14%	10%
Member of family	78%	74%	78%	65%
Family of one	10%	15%	8%	25%*
Receiving AFDC	21%	12%	24%	22%
Ever held pre-program job	69%	74%	74%	83%
Average pre-program wage	$2.04**	$2.19	$2.25	$2.72*
Atlanta sample	18%	20%	22%	31%
Boston sample	18%	23%	14%	23%
Richmond sample	23%	26%	21%	12%
San Antonio sample	26%	17%	21%	13%
Tulsa sample	15%	26%	22%	21%

*Indicates difference between attriters and those followed-up reaches statistical significance at the .05 level.

**Indicates difference between participants and comparisons at follow-up reaches significance at the .05 level.

In the second stage of this analysis, program effects on outcome measures are estimated, controlling for observable differences as well as an additional variable, lambda, representing the unobservable characteristics which affect program participation.

Table II-3 presents the results of the model predicting selection into the program. (Results of selection models run separately for the male and female samples appear in Appendix Tables A-II-5 and A-II-6.) To make these models work, the selection equation must be distinguished from the outcome equations. To do this, an additional variable must be added to the selection equation which is related to selection into the program but not to any of the outcome measures. The variables used to identify the selection equation include whether or not the youth was employed at the time of the baseline interview and the season of the year in which intake into the participant or comparison group took place. Because we control for pre-program labor market experience in our outcome models, it could be argued that employment status at the point of the baseline interview should affect program participation but not labor market outcomes two and three years later. Similarly, season of intake should affect program participation, but not subsequent labor market outcomes.

The results in Table II-3 suggest that the selection model does a fairly good job of predicting participation in the program. The overall model is statistically significant, and the identifying variables of employment status at intake and quarter of intake both have statistically significant effects. Other variables which are significant in the equations include neighborhood at intake and educational attainment. For both male and female samples, comparisons tend to have more education than participants. In the female selection equation, comparisons tend to have significantly more dependents.

Summary

Random assignment to participant and comparison groups was not used. Nonetheless, participant and comparison group samples are quite similar, in part as a result of the matching procedure utilized. Sample attrition has not biased the samples for analysis. While there may be minor observable differences between the participant and comparison groups, there may be unobserved differences as well due to the fact that there was no random assignment to the participant and comparison groups. Because of these possible differences, a correction procedure will be used in the multivariate models to control for unobserved as well as observed differences between the two groups. It should be noted that this correction procedure controls for some but not all of the unobserved differences between participant and comparison youth.

TABLE II-3

Probit Estimates of Selection into 70001 Program

	Coefficient	t-ratio
Age at intake	-.007	-.21
Ethnicity		
black	-.09	-.52
Hispanic	-.37	-1.26
Sex		
male	-.13	-1.17
Education level	-.59	-9.86*
Head of household	.22	1.28
Family of one	.05	.25
Number of dependents	-.27	-2.99*
Intake in winter	-.28	-1.59
Intake in spring	-.41	-2.88*
Intake in fall	.07	.47
Unemployment rate	-.13	-1.32
Ever held pre-program job	.19	1.56
Employed at intake	-1.20	-6.34*
Atlanta	1.03	2.28*
Boston	1.03	2.78*
Richmond	.55	2.18*
San Antonio	1.44	3.33*
Atlanta neighborhood	.33	1.61
Boston neighborhood	.50	1.96*
Richmond neighborhood	.23	1.05
San Antonio neighborhood	-.31	-1.37
Tulsa neighborhood	1.37	4.57*
Constant	2.14	3.17*

Sample size = 814
Proportion of participants in sample = 53%
Chi-square with 23 degrees of freedom = 260*

*Indicates statistical significance at the .05 level.

Notes

1) See ISR's final report to Public/Private Ventures for detailed response rates by city, sex, and participant/-comparison group. In no city was the response rate for either the participant or comparison group below 70%; 93% of the interviews were conducted in person.

CHAPTER III

PROGRAM IMPACTS

The key question that this last phase of the investigation of the effects of pre-employment programs was designed to answer was whether 70001 program effects persist in the long run or whether they decay as rapidly as did the effects of JFY. This chapter presents the results of the econometric analysis of program impacts. Alternative explanations for the effects and empirical tests of them appear in the following chapters.

Table III-1 presents mean earnings for all members of the sample regardless of whether they were employed, by participant group status and by sex, at each of the three follow-up waves. Inspection of this table suggests that at the first two waves, participants significantly outperformed comparison group members in weekly earnings. (Note, however, that this is not the case for females.) However, in the last wave, when comparing the sample aggregated by sex, participant and comparison group members do equally well. Participants had been increasing their earnings from the first to the second wave, but this rate of increase tapered off between the second and third waves, at a time when the comparison group experienced a marked increase in their average earnings. An inspection of these means by sex indicates that male participants are still outperforming their comparison group counterparts (but only slightly) whereas the female comparison group has slightly surpassed the female participants at the 24 to 40-month follow-up.

After various specifications in the multivariate analyses of the 24 to 40-month follow-up data, we found some evidence that the 70001 effects decay over time. Inspection of the means suggests that the "decay" results from the fact that comparison group members are doing increasingly better over time while participants are experiencing some slippage in their employment rate and earnings.

For the total sample, there is some empirical evidence that program effects go from being positive and statistically significant in the earliest time period to slightly negative in the final follow-up period. For females, the program effects becomes negative and statistically significant in the latter time period. For males, the program effect remains positive, but it is not statistically significant.

In this chapter, we explain how these estimates were obtained. The analysis is divided into three parts -- program effects at the point-in-time of the 24 to 40-month follow-up, program effects from calendar year 1981, and program effects over time using data from all three of the follow-up samples.

TABLE III-1

Mean Earnings at Each Follow-up Wave, by Participant Group Status and Sex

	"9"-Month[a]	"14"-Month[a]	24 to 40-Month
Total			
Participants	$46.42*	$59.24*	$62.23
Comparisons	34.45	39.18	63.32
Males			
Participants	$69.22*	$90.74*	$101.18
Comparisons	46.23	49.25	93.38
Females			
Participants	$32.90	$41.99	$37.04
Comparisons	27.21	33.18	44.16

*Indicates that the difference between participant and comparison group values reaches statistical significance at the .05 level.

[a]These numbers may vary slightly from earlier reports because the samples differ somewhat.

Program Impacts in May and June of 1982 (The Final Wave)

In this section the program's net effects at the point-in-time of the third wave follow-up interviews are estimated. As noted earlier, these interviews all took place in May and June of 1982, and varied from 24 to 40 months since the time of intake into the 70001 program. Outcome measures considered include employment status, weekly earnings, and occupational skill level. In a later chapter, we examine the effects of participation in the 70001 program on non-economic outcomes.

Table III-2 lists average wages, hours worked per week, weekly earnings, and likelihood of being employed for the aggregated sample and by sex, of the participant and comparison samples at the time of the 24 to 40-month follow-up survey.

TABLE III-2

Labor Market Outcomes at 24 to 40-Month Follow-up, by Participant Group Status and Sex

	Participants	Comparisons
Aggregated Sample		
Likelihood of being employed	.38	.42
Mean wages of those working	$4.18	$4.14
Weekly hours of those working	37	36
Weekly earnings including zero earners	$62.23	$63.32
n	451	392
Males		
Likelihood of being employed	.53	.49
Mean wage of those working	$4.55	$4.54
Weekly hours of those working	40	38
Weekly earnings including zero earners	$101.78	$93.78
n	179	154
Females		
Likelihood of being employed	.29	.37*
Mean wage of those working	$3.73	$3.74
Weekly hours of those working	34	35
Weekly earnings including zero earners	$37.04	$44.16
n	272	238

*Indicates statistical significance at the .05 level.

As the table indicates, participant males are doing slightly better in the labor market than comparison males, while participant females are doing somewhat worse than comparison females. None of these differences, however, are statistically significant with the exception that comparison group females are more likely to be employed than participant females.

To separate out program effects, multivariate models for weekly earnings and employment status were estimated. These models were estimated for the total sample and also separately for males and females. Models were estimated separately for each sex because the determinants of earnings are quite different (and program impacts diverge) for the two sexes, as many of these young women begin their childbearing during this follow-up period. The patterns that appear in Table III-1 itself provide ample justification for the inspection of models estimated separately by sex.

Individual characteristics which were held constant included age, educational status at intake, race, Hispanic ethnicity, whether or not the individual had ever held a job before the program, number of dependents at intake, whether or not the individual was a head of household or a member of a family at intake, as well as whether the individual lived in Atlanta, Boston, Richmond, San Antonio, or Tulsa. The lambda variable in these models reflects the attempt to control for selection bias using the results of the participation equation described in the previous chapter.

Some could argue that merely inspecting the effects on employment and earnings is not sufficient for the youth popultion, who during this transitional time may choose to return to school. (Therefore, analyses on schooling outcomes appear in Chapter V.) The fear is that higher proportions of youth in school could result in evidence of lower earnings in the short-run, but higher earnings in the longer run. Inspection of the schooling data suggest that the program has significantly increased the likelihood of completion of a GED since intake, but differences between participants and comparison groups on current enrollment or those who list education or training as their major activity are at the 24-40 month follow-up quite small. This suggests that our decision to analyse the data this way (and the rejection of alternative, and yet more troublesome methods) has probably not seriously biased the findings.

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* Controlling for education and training since intake is problematic because this may have been a result of program participation (and therefore is not exogenous in the outcome equation. Estimating models separately for those in school and those not in school is equally problematic. We are unable to control for differential solution into those two groups through use of the technique because no instrument was available.

Tables III-3 to III-5 present the results of the earnings models. Appendix Tables A-III-1 and A-III-2 report the results of the probability of employment models.

As is clear in these tables, we have found no evidence that the program has a positive effect on earnings at the 24 to 40-month follow-up. For the combined sample, the models suggest that the program effect is near zero and statistically insignificant; for males, the models suggest that the program effect is positive but not statistically significant; for females, the models suggest the program effect is negative and statistically significant.

The earnings models also were estimated both without the correction for selection bias and with five additional variables to take into account the fact that participants and comparisons were differentially geographically distributed within the five cities. The conclusions made about the decay of program effects are not sensitive to whether we control for selection bias. Omitting the correction for selection bias does not change the program effect for the total sample, but it evens out somewhat the difference between program impacts on males and females (although the effect for females is still negative and statistically significant). Adding the five neighborhood variables lowers the program effect for all three samples. The results of models without the correction for selection bias are presented in Appendix Tables A-III-3 and A-III-4.

Of the other explanatory variables in these models, it is a bit surprising that education has no effect for males and is negative and statistically significant for females. However, it should be kept in mind that all of the youth in the sample are high school dropouts. For males, there may not be that much difference between leaving school after 9th, 10th or 11th grade. For females, the negative education effect may be due to the fact that women who left school early to have children, now have children old enough to permit re-entrance to the labor market. The effect of most of the other explanatory variables are as expected.

We also investigated whether there was evidence that for certain subgroups of the participants, the program still had positive and statistically significant effects at the 24 to 40-month period. Comparisons were made for those of different racial/ethnic groups (white/black/Hispanic) and participants of different ages (under 18 at intake versus 18 years of age and older). In none of these groups could there be found empirical evidence for statistically significant positive program effects on employment and earnings in the 24-40 month period.

TABLE III-3

Adjusted Tobit Estimates of the Determinants of Weekly Earnings at 24 to 40-Month Follow-up, Total Sample

Independent Variables	Adjusted Coefficient[a]	t-ratio
Age at Follow-up	1.15	1.12
Race (Black = 1)	-33.56	-6.99*
Ethnicity (Hispanic = 1)	-41.19	-5.25*
Sex (Male = 1)	40.29	12.07*
Education at intake	-2.06	-.90
Head of household at intake	-7.32	-1.34
Not living with family at intake	14.68	2.91*
Dependents at intake	3.07	1.03
Ever held pre-program job	10.76	2.98*
Atlanta	-9.95	-1.85
Boston	-1.43	-0.27
Richmond	-21.79	-4.05*
San Antonio	14.41	1.86
Lambda (Correction variable)	3.56	.62
Participation	-7.02	-.79
Constant	1.02	.05

Sample size = 828
Mean weekly earnings = $62.72
Proportion with positive earnings = .40
Sigma = 39.78

[a]The tobit coefficients have been adjusted by the proportion in the sample with positive earnings.

*Indicates statistical significance at the .05 level.

TABLE III-4

Adjusted Tobit Estimates of the Determinants of Weekly Earnings at 24 to 40-Month Follow-up, Males[a]

Independent Variables	Adjusted Coefficient[b]	t-ratio
Age at Follow-up	3.86	-.92
Race (Black = 1)	-41.36	-2.07*
Ethnicity (Hispanic = 1)	-48.38	-.90
Education at intake	.95	.09
Head of household at intake	-59.73	-1.48
Not living with family at intake	31.24	1.42
Dependents at intake	19.89	.90
Ever held pre-program job	28.78	1.91
Atlanta	-8.96	-.39
Boston	17.66	.71
Richmond	-42.46	-2.40*
San Antonio	50.18	.91
Lambda (Correction variable)	-19.68	-.84
Participation	31.39	.84
Constant	76.12	-.93

Sample size = 324
Mean weekly earnings = $97.22
Proportion with positive earnings = .54
Sigma = 96.09

[a]Here, method of moments technique is used to derive the tobit coefficients as the Fletcher Powell procedure did not converge. It should be noted that the Fletcher Powell routine produces more statistically significant effects than the method of moments, a less efficient routine.

[b]The tobit coefficients have been adjusted by the proportion in the sample with positive earnings.

*Indicates statistical significance at the .05 level.

TABLE III-5

Adjusted Tobit Estimates of the Determinants of Weekly Earnings at 24 to 40-Month Follow-up, Females

Independent Variables	Adjusted Coefficient[a]	t-ratio
Age at Follow-up	.51	.54
Race (Black = 1)	-37.69	-8.03*
Ethnicity (Hispanic = 1)	-46.19	-6.63*
Education at intake	-5.03	-2.40*
Head of household at intake	2.23	.53
Not living with family at intake	5.00	.89
Dependents at intake	-4.90	-2.05*
Ever held pre-program job	8.61	2.86*
Atlanta	13.38	-2.78*
Boston	-3.85	-.73
Richmond	-11.91	-2.37*
San Antonio	2.50	.37
Lambda (Correction variable)	18.23	3.46*
Participation	-31.65	-3.83*
Constant	55.78	2.59*

Sample size = 504
Mean weekly earnings = $40.54
Proportion with positive earnings = .32
Sigma = 19.53

[a]The tobit coefficients have been adjusted by the proportion in the sample with positive earnings.

*Indicates statistical significance at the .05 level.

We also examined the occupational skill distribution of the participant and comparison samples at 24 to 40-months after--intake. Occupations from the survey were coded into three-digit census classifications and then ranked by their Specific Vocational Preparation (SVP) score. The SVP index consists of nine levels, denoting the time it takes to become proficient at an occupation once prerequisite education has been completed. The scale varies from level 1 (short demonstration only) to level 9 (over 10 years). Here, a skilled occupation was defined as one requiring at least six months to learn properly (SVP level 5 and above). Examples of occupations classified as skilled under this definition include carpenters, machinists, mechanics, and apprentices in the construction trades. Examples of occupations classified as unskilled include deliverymen, health aides, laborers, and operatives. Table III-6 lists the percentages of the employed comparison and participant samples who were in skilled occupations at the time of their third wave interview.

TABLE III-6

Percentages of Employed Participants and Comparisons in Skilled Occupations at 24 to 40-Month Follow-up, by Sex

	Participants	Comparisons
Males	25%	21%
n	100	82
Females	5%	19%*
n	80	81

*Indicates that difference between participants and comparisons reaches statistical significance at the .05 level.

Table III-6 indicates that male participants in 70001 were not more likely than male comparison individuals to hold skilled jobs at the 24 to 40-month follow-up. However, comparison group females who were employed were significantly more likely to be found in skilled jobs than participant females. A multivariate model was estimated to explain employment in skilled occupations. From these multivariate analyses, we found no evidence that program participation significantly improved the chances of youth's moving into skilled occupations.

These results are not surprising as most of the youth in the sample are employed in low-skilled occupations. For both participant and comparison males, most commonly held jobs included janitors, food service workers, short-order cooks, deliverymen, gardeners, and security guards. For participant and comparison

females, most frequently held jobs included food service workers, cashiers, nurse's aides, cleaners and janitors, and office/clerical workers. Occupational distributions, by participant group status and by sex, are presented in Appendix Tables A-III-5 and A-III-6.

Program Impacts During the 1981 Calendar Year

In the third wave follow-up interviews, besides obtaining information on current labor market status and earnings, questions were also asked regarding employment and earnings for calendar year 1981. As participants entered the program sometime between January 1979 and April 1980, calendar year 1981 represented a period that for the most part ranged between 12 and 33 months since intake.

We were interested in inspecting calendar year earnings because at each previous wave of follow-up, we had only obtained information on weekly earnings and were, therefore, forced to make assumptions about how these translated into annual earnings gains. We therefore collected data on annual earnings in hopes of obtaining more in-depth estimates of program impacts. A caveat is appropriate here, however. Since calendar year 1981 was between 12 and 33 months after intake, analyses of annual earnings will not be directly comparable to any of the wave-by-wave analyses. In fact, the effects might be expected to be somewhere between those found in the second and third waves.

Table III-7 lists averages for yearly earnings, weeks worked full-time, and weeks worked part time, by sex, for the participant and comparison samples as reported for 1981.

As is evident in the table, both participant males and females did better than their respective comparison groups during 1981 than they did in May and June of 1982. While most of these differences are not statistically significant, male participants worked substantially more full-time weeks in 1981 than did comparison males. (Note that participant relative to comparison group ones may be slightly underestimated due to differential school enrollment.)

We attempted to separate out program effects on these labor market outcomes through the use of multivariate analysis. We found no evidence that program participation had a significant effect on 1981 earnings or weeks worked full-time for males or females.

We also examined program effects on the skill level of the job held longest during 1981. Table III-8 lists the percentages of employed participants and comparisons, by sex, who had skilled occupations at their longest held jobs in 1981. Table III-8 suggests that participants were not more likely than comparisons

TABLE III-7

Labor Force Outcomes Reported for Calendar Year 1981, by Participant Group Status and Sex

	Participants	Comparisons
Males		
Mean yearly earnings	$4,486	$3,891
Mean weeks worked full-time	27.0*	19.9
Mean weeks worked part-time	5.0	4.8
n	179	154
Females		
Mean yearly earnings	$1,813	$1,747
Mean weeks worked full-time	13.3	12.3
Mean weeks worked part-time	3.3	3.5
n	272	238

*Indicates statistical significance at the .05 level

TABLE III-8

Percentages of Employed Participants and Comparisons Who Had Skilled Occupations in the Job Held Longest During 1981

	Participants	Comparisons
Males	21%	22%
n	132	101
Females	10%	17%
n	131	94

to be employed in skilled jobs in 1981. No evidence was found that the program had a statistically significant impact on occupational attainment during calendar year 1981.

Thus, we have found no evidence that participation in the 70001 program had a statistically significant impact on earnings or employment 24 to 40 months after entry into the program or on earnings and employment during calendar year 1981, some 12 to 33 months subsequent to entry to the program.

Program Effects Over Time

In this section, we use data from the three follow-up waves to estimate changes in program effects over time. Across the three waves of data, follow-up observations range from 7 to 40-months since intake. The spread is fairly evenly distributed throughout the entire range. To test for changes in program effects over time, the months-since-intake variable was broken into intervals of six months each.

Table III-9 summarizes the changes over time in program effects on weekly earnings. There is no "best" method to estimate effects over time in a pooled data set when the distribution of the dependent variable requires maximum likelihood estimation techniques. For this reason, estimates using alternative techniques are presented. (Appendix B explains how these estimates were derived and the limitations of each.) Table III-9 assesses the sensitivity of the estimates and conclusions to two factors: the inclusion of lambda a correction for unobserved differences between the participant and comparison groups), and whether the data are pooled or estimated wave by wave. We prefer estimates corrected for selection bias; but we have no preferred estimation technique between the pooled and the wave-by-wave, since the pooled estimates are uncorrected for correlations of errors across observations associated with the same individuals.

While the estimates fluctuate considerably, most of the conclusions drawn from them do not seem to be very sensitive to the estimation procedure. They are as follows:

- When considering the sample aggregated by sex, program participation appears to have a statistically significant positive effect on participants in the early post-program periods, but then the effect becomes negative, but not statistically significant. Thus there is evidence that the overall program effect exists but decays.

- For males, there is evidence that: the program has a larger positive, and almost always statistically significant effect in the early post-program periods, but the effect tapers off and becomes statistically insignificant and negative in some model specifications in certain

TABLE III-9

Adjusted Estimates of the Net Effects of the 70001 Program[a] on Earnings by Months Since Intake and Sex

	7-12	13-18	19-23	24-29	30-35	36-40
Males						
Wave by wave (corrected for selection bias)	24	52	22	29	39	13
Pooled (corrected for selection bias)	27*	42**	30*	32*	21	9
Wave by wave (not corrected for selection bias)	16	53**	22	-2	11	-15
Pooled (not corrected for selection bias)	22**	36**	25**	26*	16**	-14
Females						
Wave by wave (corrected for selection bias)	7	-27**	-30**	-30**	-43**	19*
Pooled (corrected for selection bias)	-9*	-5	-9	-20**	-24**	-11*
Wave by wave (not corrected for selection bias)	6	10**	7	-2	-14**	12**
Pooled (not corrected for selection bias)	5*	9**	5	-5	-9**	4
Total[b]						
Wave by wave (corrected for selection bias)	15**	13	2	-7	-12	-8
Wave by wave (not corrected for selection bias)	10**	29**	16**	1	-5	-.4

*Indicates statistical significance at the .10 level.

**Indicates statistical significance at the .05 level.

[a]Appendix B describes how these estimates were obtained. Coefficients have been adjusted by the proportion about the limit.

[b]Note that the pooled models could not be estimated for the total data set.

time periods. Thus, there is evidence that the effect for males exists in the early post-program period and then decays.

- For females, there is evidence that the effect is negative and statistically significant in the later post-program periods.

The estimates of the effects for females in the early post-program periods are sensitive to the estimation procedures used.

Thus, while the estimates during the latter three time periods are not sensitive to the type of estimation procedures used, some of the earlier estimates are. Nevertheless, there is evidence of a decay of program effects at 24 to 40 months after program intake.

Summary

In this chapter we inspected program effects at the time of the 24 to 40-month follow-up, for calendar year 1981, and for the entire 7 to 40-month follow-up period. For males, we found evidence that the program effects were positive but statistically insignificant in the 24 to 40-month post-program period. We found additional evidence that program effects were found to be negative and statistically significant for females, and slightly negative but not statistically significant for the total sample at the 24 to 40-month follow-up. For calendar year 1981, program effects were insignificant for all three groups. Over the 7 to 40-month follow-up period, there is evidence that program effects were found to have decayed for all three samples. The effect for males remains positive but is not close to statistical significance. No significant differences in occupational attainment were found between the participant and comparison groups -- either at the time of the 24 to 40-month follow-up or during calendar year 1981. Three important factors must be kept in mind when considering these results. First, the programs selected for follow-up were not representative of all 70001 programs. Second, these results address the longer term effects of 70001 on enrollees in the programs several years ago. It is possible that modification to the program have altered the pattern of program impact. Third, while not likely, it is remotely possible that differential enrollment in educational training programs effects these results in an unknown fashion. (This latter possibility is investigated in Chapter V.)

The next chapter investigates possible explanations for the decay of program effects. First, potential effects of the recession are inspected and then differential childbearing by participants and comparison group females is assessed.

CHAPTER IV

ASSESSMENT OF ALTERNATIVE EXPLANATIONS

In Chapter III, we found evidence that the effects of the 70001 program had decayed over time. One possible explanation is that the latest observations were obtained in the middle of a deep recession, which hit youth and inner cities particularly hard. In this chapter, we attempt to disentangle the impacts of the recession on the estimates of program effects. Further, the program has negative effects on females in the longer run post program periods. This is a bit surprising. Therefore, this chapter attempts to make sense of this finding by ascertaining whether differential childbearing by participant and comparison females has produced the negative program impact for females. After the assessment of alternative explanations, the summary of the chapter concludes the analysis of post-program impacts.

EFFECTS OF THE RECESSION ON PROGRAM EFFECTS

One concern is that the 24 to 40-month follow-up interviews took place in May and June of 1982 in the midst of a deep recession. The comparison group methodology should take care of this problem since participant and comparison youth should be similarly affected by the economic downturn. However, a case could be made that the participant and comparison groups were differentially affected by the recession: the participant group could have moved into better paying and higher skilled jobs if it were not for the bad economic times; or the participant group went into the recession with higher employment rates and thus had more to lose. We attempt through a number of different approaches to disentangle program decay effects from the effects of a recessionary economy.

The most straightforward approach to trying to separate out recessionary effects is simply to look at the patterns of employment changes that have occurred in the participant and comparison samples. Table IV-1 breaks down mean earnings at each of the follow-up waves into wage and employment components.

When the effects of a program "decay," it can result from comparison group gains over time, deterioration in gains of participants, or both. If the recession was largely responsible for the decay of program effects, we would expect to see little improvement by comparison group members and a deterioration of the participant group position. The patterns exhibited in Table IV-1 make it difficult to accept a simple recessionary effect explanation for the decay of program effects. Participants do suffer employment losses between the second and third follow-up waves, but a large part of the decay of program effects is due to comparison gains rather than participant losses. The patterns in Table IV-1 do not appear to suggest recessionary effects, but rather decay of program effect.

TABLE IV-1

Proportion Employed and Mean Wage Rates
at Each Follow-up Wave, by Participant Group Status and Sex

Proportion Employed[a]

	Follow-up wave		
	"9" month	"14" month	"24-40" month
Males			
Participants	.55*	.60*	.53
Comparisons	.37	.43	.49
Females			
Participants	.32	.33	.29*
Comparisons	.25	.31	.37
Total			
Participants	.41*	.43	.38
Comparisons	.29	.36	.42

Wage Rates for Those Who Are Employed[a]

Males			
Participants	$3.60	$4.04	$4.55
Comparisons	$3.60	$3.64	$4.54
Females			
Participants	$3.20	$3.59	$3.73
Comparisons	$3.45	$3.53	$3.74
Total			
Participants	$3.40	$3.81	$4.18
Comparisons	$3.52	$3.57	$4.14

*Indicates difference between participants and comparison group reaches significance at the .05 level.

[a]These numbers may vary slightly from those in earlier reports because the samples differ slightly.

A second fairly simple test for possible recessionary effects is to concentrate on particular follow-up waves in looking for program decay. The 24 to 40-month follow-up wave took place entirely in May and June of 1982 in the middle of a recession. The "14" month follow-up wave had a mean of 15 months since intake, but spanned a period of 7 to 29 months since intake. Many of the interviews took place before the recession began. Table IV-2 presents estimates of program effects by sex and follow-up wave, by the number of months elapsed since intake.

TABLE IV-2

Adjusted Estimates of Net Effect of the 70001 Program on Earnings at the "14" Month and 24 to 40-Month Follow-up Waves, by Sex

"14" Month Wave

	7-18 Months	19-29 Months
Males	53	25
Females	-29*	-34*

24 to 40-Month Wave

	24-32 Months	32-40 Months
Males	41	12
Females	-35*	-33*

*Indicates that the effect itself reaches statistical significance at the .05 level.

The results indicate that, at least for males, there is some evidence of decay of program effects independent of possible recessionary effects. Even though the 24 to 40-month follow-up interviews took place at the same point in time, the program appears to have a larger impact on males with shorter times since intake than on males out of the program a longer period of time. This holds true for males in the "14" month wave as well. For females the trends are less clear.

A third approach to separate out possible recessionary effects is to examine the occupational and industrial distribution of the participant and comparison groups for the job they held longest in calendar 1981. While the 1981 calendar year was a recessionary period, the recession was less severe than it was in May or June of 1982. Participant youth may have been in more skilled occupations or in more sensitive industries, and thus more likely to lose their jobs because of the recession. Table IV-3 compares the percentages of employed sample youth, by participant group status and by sex, in skilled occupations in calendar 1981 and at the time of their 24 to 40-month interviews in 1982. Table IV-4 lists the industry distribution of employed sample youth, by participant group status and by sex, for the job held longest during 1981.

TABLE IV-3

Percentages of Employed Sample Youth
Who Were in Skilled Occupations in the Job
Held Longest in 1981 and in May and June of 1982,
by Participant Group Status and by Sex

	Job Held Longest in 1981	Job in May and June of 1982
Males		
Participants	21%	25%
Comparisons	22%	21%
Females		
Participants	10%	5%*
Comparisons	17%	19%

*Indicates that difference between participants and comparison group members reaches statistical significance at the .05 level.

TABLE IV-4

Industry Distribution of Sample Youth at Job Held Longest in 1981, by Participant Group Status and by Sex

	Participants	Comparisons
Males		
Agriculture, forestry, mining	2%*	2%
Construction	8%	13%
Durable goods manufacturing	10%	13%
Non-durable manufacturing	6%	13%
Transportation and Communications	5%	5%
Wholesale trade	4%	6%
Retail trade	27%	19%
Finance and Insurance	4%	2%
Personal services	5%	2%
Business services	11%	14%
Entertainment & Recreation Services**	2%	0%
Professional & Related Services**	10%	9%
Public Administration	8%	2%
Females		
Agriculture and mining	2%	1%
Construction	1%	1%
Durable goods manufacturing	5%	2%
Non-durable manufacturing	11%	16%
Transportation and Communications	1%	2%
Wholesale trade	1%	2%
Retail trade	33%	22%
Finance and Insurance	7%	4%
Personal services	8%	5%
Business services	4%	10%
Entertainment & Recreation Services	0%	3%
Professional & Related Services**	22%	24%
Public Administration	3%	5%

*Percents may not add to 100 due to rounding.
**i.e., Theaters, hospitals, convalescent homes, doctor's offices etc.

Table IV-3 suggests no marked changes between 1981 and 1982 in the occupational position of participants relative to comparisons. The occupational position of female participants deteriorates somewhat relative to comparisons, but even in calendar year 1981 female participants were doing quite poorly in terms of occupational attainment. Table IV-4 reveals some differences in industry distribution between participant and comparison youth for calendar year 1981. However, participant youth were not more likely to be employed in either construction or durable goods manufacturing -- two industries particularly hard hit by this recession. Neither of these tables lends support to the notion that were it not for the poor economy participants would have been in much higher earning positions.

A fourth approach to disentangling program and recessionary effects is to look at changes over time in program effects by city. The recession had differing impacts across the five cities in the sample. At the time of the 24 to 40-month follow-up survey, local total unemployment rates for cities in the sample varied from 6.1 percent in Tulsa to 9.1 percent in Boston. If the recession had differential impacts on the participant and comparison groups, program effects should last longer in Tulsa than in Boston, with the other three cities falling in between.

Two modeling strategies were attempted to disentangle differential effects in cities. First, we estimated models separately by city at each wave. Second, we interacted program effects with city at each wave. Unfortunately, the effects were sensitive to the model specification utilized. Further, effects for cities were not statistically significant and were highly variable. The test for differences between cities was not statistically significant. This investigation did not prove fruitful probably because of fairly small sample sizes in cities.

A final approach taken to separate out recessionary effects was to examine the labor market situation of youth in the general population during the same time period as the 24 to 40-month follow-up. If the bottom completely dropped out of the labor market for all non-college youth during the spring and summer of 1982, then a case could be made that 7ØØØ1 youth had no chance to begin moving into better paying, higher skilled occupations. If, however, some non-college youth still were able to obtain and then maintain higher skilled jobs even in the face of the recession, 7ØØØ1 participants should have had a chance at least to enter some skilled occupations.

To look at youth in the general population, we used the March 1982 Current Population Survey (CPS). For our purposes, the CPS sample is restricted to non-college, out-of-school youth between the ages of 19 and 23. Labor market outcomes examined include current employment status, current occupation, total earnings in calendar 1981, and occupation at job held longest in 1981. We

compare outcomes for these 19 to 23 year old youth in 1982 with the outcomes of similarly aged youth in the 1979 CPS sample. We also compare outcomes of 16 to 19 year old youth in 1979 with the outcomes of 19 to 23 year old youth in 1982. The results are presented in Table IV-5 and IV-6.

A caveat is appropriate here. We are not proposing the CPS non-college sample as a comparison group for the 70001 participants. The CPS sample defined here has a much lower proportion of disadvantaged, inner-city or minority youth than the 70001 sample. The only aim here is to examine how the recession in 1982 affected the general labor market situation of non-college, out-of-school youth.

TABLE IV-5

Labor Market Outcomes of 19 to 23 Year Old Non-College Out-of-School Youth in 1979 and Similar Youth in 1982, by Sex

	1979	1982
Males		
Proportion employed	.83	.73
Proportion currently in skilled occupations	.29	.28
Mean earnings last year	$9,546	$8,615
Proportion earning more than $10,000 last year*	.37	.33
Proportion in skilled occupations last year	.30	.29
Sample size	3,517	3,687
Females		
Proportion employed	.59	.50
Proportion currently in skilled occupations	.30	.25
Mean earnings last year	$6,154	$5,830
Proportion earning more than $10,000 last year*	.06	.16
Proportion in skilled occupations last year	.29	.24
Sample size	2,398	1,070

*1979 earnings have been adjusted to 1982 levels to take into account inflation.

TABLE IV-6

Labor Market Outcomes for 16 to 19 Year Old Non-College Out-of-School Youth in 1979 and Similar 19 to 23 Year Old Youth in 1982, by Sex

	1979	1982
Males		
Proportion employed	.76	.73
Proportion currently in skilled occupations	.16	.28
Mean earnings last year	$3,805	$8,615
Proportion earning more than $10,000 last year	.08	.33
Proportion in skilled occupations last year	.15	.29
Sample size	3,187	3,687
Females		
Proportion employed	.67	.55
Proportion currently in skilled occupations	.12	.25
Mean earnings last year	$2,600	$5,655
Proportion earning more than $10,000 last year	.02	.14
Proportion in skilled occupations last year	.12	.25
Sample size	3,117	4,028

*1979 earnings have been adjusted to 1982 levels to take into account inflation.

Table IV-5 helps to show the extent to which the recession has affected non-college youth in the 16 to 23 year old age cohort. As compared to 1979, youth in this age cohort now have considerably lower employment rates and slightly lower representation in skilled occupations. However, the bottom has not dropped out of the labor market for these youth. The majority of youth in the sample were employed as of March 1982, and at least a fair percentage are in higher-skilled occupations.

Table IV-6 helps illustrate the movement of the 16 to 19 year old age cohort in 1979 from their earlier position in the labor market to their current position as 19 to 23 year olds. While the percent employed has declined somewhat, youth have progressed

to jobs with higher skill levels, in spite of probably being held back by the recession. There is some evidence that the recession is impacting on the employment opportunities of this age group.

Summary of Recessionary Effects

In this section several approaches were taken in an attempt to disentangle recessionary and program decay effects. Inspection of data from the CPS suggests a deterioration in labor market position for all youth of this age group. The fact, however, that participants were not more likely to be in skilled occupations in 1981 -- nor more likely to be in sensitive industries such as construction or durable goods manufacturing -- casts some doubt on the premise that without the recession, participants would be found in much higher earning positions. Also, the fact that the position of participant group members is deteriorating or at least not improving, at the same time as the position of members of the comparison group is improving substantially, coupled with all of the findings in this section, suggests that the recession was not, in itself, the cause of the decay of program effects.

SEPARATING CHILDBEARING AND PROGRAM EFFECTS

It is troublesome that labor market effects of the program are negative and statistically significant for females at 24 to 40 months post-intake. It is possible that participant females had more children in the follow-up period than comparison group females, and were thus less likely to be found in the labor market. Table IV-7 shows changes in the mean number of dependents between intake and the 24 to 40-month follow-up period for participant and comparison females.

TABLE IV-7

Mean Number of Dependents of Females at Intake and at 24 to 40 Month Follow-up, by Participant Group Status

	Intake	24-40 Month Follow-up
Participants	.49*	.97
Comparisons	.65	1.07

*Indicates difference between participants and comparisons reaches statistical significance at the .05 level.

Table IV-7 indicates that participant and comparison women had children at roughly the same rate during the follow-up period with each group adding 0.5 children. Thus, the gap in mean number of children has closed in percentage but not in absolute terms. Table IV-8 provides more information on childbearing patterns. It reveals a striking similarity in childbearing propensities during the follow-up period. The differences that remain at follow-up are not due to differential childbearing during the follow-up period, but rather a continuation of the differences that existed at intake.

TABLE IV-8

Childbearing Statistics for Participant and Comparison Females at Intake and at 24 to 40-Month Follow-up Period

	Participant	Comparison
Percent with children at intake	40%	48%*
Percent having children during follow-up period	42%	41%
Percent having first child during follow-up period	23%	22%
Percent having more than one child during follow-up period	6%	5%
Mean number of children per woman born during follow-up period	.50	.48
Percent with children at follow-up	63%	67%
Mean dependents per woman at follow-up	.97	1.07

*Indicates statistical significance at the .05 level.

One logical explanation for why participant females had fewer children at intake than comparisons is that they were simply younger. Table IV-9, presenting the age distribution of the women in the participant and comparison samples, shows that this is the case. While the mean age of the two groups is similar, the actual age distributions are not: 45% of participant females were 17 or younger at intake as opposed to 28% of the comparison group.

TABLE IV-9

Age Distribution in Percents of the Female Particpant and Comparison Samples at Intake

Age	Participant	Comparison
15	.6%	3%
16	17%	9%
17	27%	16%
18	20%	24%
19	14%	23%
20	13%	16%
21	7%	6%
22	.6%	2%
23	.3%	.4%

Note: Percentages may not add to 100% due to rounding.

It might be of interest to ascertain how childbearing during the follow-up period affected program participation effects and labor market outcomes; however, this is difficult. On the one hand, it is standard procedure to use current values of explanatory variables only for those factors that could not possibly be affected by program participation. Theoretically, program participation could affect childbearing. Also earnings and employment can affect fertility causing "endogeneity" problems -- problems caused by introducing an independent variable that can be affected by the dependent variable. On the other hand, outcome models for females using age and dependents at intake as explanatory variables will not fully capture the fact that female participants, being younger, had a lower proportion of children at intake, relative to comparisons, than they did at follow-up. To the extent that natural aging effects rather than program participation caused a narrowing of the differential levels of dependents present between the participant and comparison groups, such a model could affect the estimates of the program participation effect.

To test for possible program effects on childbearing, a model was estimated explaining changes in number of dependents between intake and the 24 to 40-month follow-up period to investigate whether program participation had effects on fertility. Explanatory variables in the model include age, race, Hispanic ethnicity, pre-program work experience, dependents at intake,

family status at intake, pre-program education, site, and months elapsed between intake and follow-up. Program participation did not have a statistically significant effect on childbearing during the follow-up period.

Given that program participation does not appear to be related to childbearing, there is some justification for entering current number of dependents rather than dependents at intake in the outcome models for women. Note, however, that this is not technically correct since labor market outcomes (the dependent variable) can cause changes in an independent variable (number of dependents). However, even current number of dependents does not fully capture the fact that participants, having had a greater proportion of their children since intake, may have children who are younger on average than the children of the women in the comparison group. To try to take into account possible differences in children's ages, we ran an earnings model that included both number of dependents at intake and changes in the number of dependents since intake. The results suggest that even with a variable included for children since intake, program participation continues to have a negative and statistically significant effect for females. (Compare Model 1 and Model 2 in Table IV-10.)

In a final attempt to deal with the intermingling of childbearing and program effects on employment and earnings, earnings models were estimated separately for women with children at the 24 to 40-month follow-up period and women without children at that time. It should be noted that this does not fully deal with the problem of possible program effects on fertility. Instead, it simply assumes that the program had no effect on childbearing. Separating the sample into women with children and women without children, however, circumvents the endogeneity problem of including current number of children (or number of children born since intake) in a model explaining employment or earnings. Table IV-10 summarizes the results of the various attempts to deal with the issue of childbearing in the earnings models for women. Program participation continued to have a negative and statistically significant effect for women with children. However, for women without children at follow-up, although the effect is still negative, it lacks statistical significance.

As is clear from the Table IV-10, no matter how we model childbearing since intake, the program lacks a positive effect on earnings for females. In all cases, except for women with no children at follow-up, there is evidence that the effect is negative and statistically significant.

TABLE IV-10

Adjusted Estimates of Program Effects
Derived from Tobit Models on Female Earnings
Using Alternative Independent Variables and Different Samples
to Tap Childbearing Effects

	Estimate of Program Effect	t-ratio
Model 1[a,b]		
Dependents at Intake	-31.65	-3.83*
Sample: all females		
Model 2[a,b]		
Dependents at Intake and changes in number of dependents	-33.19	-4.04*
Sample: all females		
Model 3[a,b]		
Dependents at Intake		
Sample: Only women with children at follow-up	-48.58	-5.27*
Model 4c[a,b,c]		
Sample: Only women with no children at follow-up	-9.80	-.58

*Indicates estimates which are statistically different from zero at the .05 level.

[a]These models also include the full roster of independent variables.

[b]Estimates are adjusted by the proportion of the sample working (above the limit).

[c]Note, dependents at intake is excluded from this model because all females in this model have a value of zero.

It is possible that we have not fully captured the fact that participants, being younger at intake, now are more likely to be pregnant. However, responses in the 24 to 40-month survey indicate that the same proportions of participant and comparison females are currently pregnant (see Table A-III-7 in the Appendix). It is also possible that we have not fully captured the fact that participants, being younger at intake, now have a higher proportion of children under six. There is some support for this in survey responses in that a higher proportion of female participants report family responsibilities and day care problems as reasons for being out of the labor force. (Again, see Table A-III-7 in the Appendix.)

Overall, this section suggests that childbearing (or differential childbearing on the part of two groups) does not fully explain the negative effects of the program on participants. Even for females with no children during the follow-up period, we find no evidence of positive effects of the 70001 program.

Chapter Summary

We found evidence in Chapter III that the effects of the program on weekly earnings for both male and female participants and the combined sample have decayed relative to their comparison groups. In this chapter, we examined the extent to which the recession and childbearing among females in the sample may have been responsible for these patterns of program effects. While there is some evidence that both the recession and childbearing may have affected program results, it appears that most of the change over time in participant and comparison earnings differentials is due to the decay of program effects.

The general pattern of effects is fairly easy to explain. Both participant and comparison youth started out unemployed with zero weekly earnings. The participant youth were placed by the program and opened a wide differential between themselves and comparison youth in both the percentage employed and weekly earnings. Over time, the participant youth gradually left or lost their jobs and the comparison youth gradually found jobs on their

own or through other agencies. If the program has no lasting effect on participant employability, skill level, or labor force attachment, the participant and comparison groups -- assuming they are roughly similar -- will eventually attain the same level of employment and equivalent weekly earnings.

The fact that participant females end up doing worse than comparison females is a little more difficult to explain. One possibility is that youth, particularly females, do better in the labor market when they find jobs for themselves, rather than being placed by a program such as 70001. Another possibility is that there still remain some childbearing effects -- such as the age of youngest child -- which we have not been able to control for adequately in the analyses.

CHAPTER V

OTHER OUTCOMES

Aside from affecting labor market outcomes such as employment and earnings, participation in the 70001 program could have effects on a variety of other aspects of the lives of youth. Participation in the program could encourage entry into the military or re-entry to school. The jobs provided by the program could alter the social and economic situations of youth, and either speed up or delay marriage and leaving home. The jobs provided by the program also could help keep youth out of trouble with the law. Table V-1 summarizes the post-program distribution of participant and comparison youth on these other dimensions.

As Table V-1 indicates, participants are significantly more likely than comparisons to have GED certificates, while comparisons are significantly more likely than participants to have regular high school degrees at the 24 to 40-month follow-up. Participants are also more likely (and statistically significantly more likely in the case of males) than comparison group members to have <u>either</u> a GED or high school degree. Recall from Table II-1 that only one percent of the participant and comparison samples had a high school degree or equivalent at intake. Thus, nearly all of the youth in either sample who have degrees obtained them after program intake.

This increase in education obtained could be considered as a program benefit. Regression models were estimated inspecting the effects of background characteristics and program participation on years of education completed at follow-up (where GED and high school degree were both counted as 12 years of schooling). We found evidence that program participation had a significant and positive effect on increases in education, chiefly in the form of GEDs, suggesting that the program is successful in encouraging participants to continue their education. It should be noted that this difference between participants and comparisons could in part be simply the result of self-selection. One of the attractions of the 70001 program is that it offers a chance to pursue a GED. Individuals who enroll in the program probably tend towards placing particular value on having a GED certificate.

Given that program participants are more likely to obtain GEDs and comparison group members are more likely to receive regular high school degrees, it is somewhat ambiguous as to whether this is a program benefit. If GEDs and regular high school degrees have the same payoff in terms of subsequent earnings, then the fact that more participants receive some form of high school degree is a program benefit. If, however, the subsequent earnings payoff for a GED is substantially less than that of a regular degree, then perhaps this is not a program benefit at all.

TABLE V-1

Characteristics at 24 to 40-Month Follow-up of Participant and Comparison Youth, by Sex

	Participants	Comparisons
Males		
Military: Active duty	3%	1 %
Reserves	3%	3%
In jail	6%	6%
Have GED	31%	12%*
Have regular high school diploma	3%	9%*
Have either GED or regular high school diploma	34%	21%*
Married	15%	16%
Living with parents	43%	43%
Receiving support from parents	41%	42%
n	179	154
Females		
Military: Active duty	.7%	0%
Reserves	.4%	.4%
In jail	0%	0%
Have GED	21%	13%*
Have regular high school diploma	8%	12%*
Have either GED or regular degree	29%	26%
Married	20%	26%
Living with parents	38%	39%
Receiving support from parents	33%	33%
n	272	238

*Indicates difference between participant and comparison groups reaches statistical significance at the .05 level.

It is interesting to note that the program does not appear to increase the percentage of male participants entering the military. Sample size limitations make it difficult to make a judgment on the effect of 70001 on military enrollment. Simply looking at youth on active duty, three percent of participant males and one percent of comparison males are in the military. Looking at proportions of those who have been (or are) on active duty, reserves and veterans since intake, the corresponding numbers are seven percent and six percent. In the Job Corps study, Mathematica found that comparison males had a four percent probability of being in the military and participant males had a nine percent probability of being in the military, with the difference being statistically significant. Clearly 70001 is not helping youth get into the military at the rate that the Job Corps is. In 70001 the actual numbers are quite small and the differences are not statistically significant. For females, five participants and one comparison were on active duty, in the reserves, or a veteran since intake. But the five participants reflect only two percent of the female participant sample, and so it can hardly be said that 70001 is moving large numbers of females into the military. One of the main reasons the Job Corps appears to have an enduring effect on the earnings of male participants is that it increases enlistment in the armed forces.

It is possible that the 70001 program affects the decisions of youth to enter other educational and training programs. On the one hand, participation in the classroom training element of the program may increase awareness of educational options available. On the other hand, the job placements provided by the program may serve as an alternative to continuing education or training.

Table V-2 presents school or training enrollment in each of the three follow-up waves. Table V-3 examines past participation in educational and training programs. Table V-4 inspects the types of educational and training programs in which the youth were currently enrolled at the third follow-up interview. The three tables indicate that there is some difference in the past and current enrollment of participants and comparisons in educational and training programs. The differences in educational attainment, however, do not appear particularly striking, nor are they statistically significant.

Because a slightly larger number of participants are currently enrolled in school or training, this raises some questions as to whether this could contribute to a misleading low estimate of long run program effects on earnings. However, the number of sample youth who list education and training as their current main activity is quite small. Only four percent of participant males, one percent of comparison males, four percent of participant females, and two percent of comparison females list school or training as their reason for being out of the labor force. (See Table A-III-7 in the Appendix).

TABLE V-2

Enrollment in Educational or Training Programs at Each Follow-up Wave, by Participant Group Status and Sex**

	Participants	Comparisons
Males		
"9"-month	19%	11%
"14"-month	20%	6%*
24 to 40-month	14%	12%
Females		
"9"-month	23%	11%*
"14"-month	15%	10%
24 to 40-month	17%	13%

*Indicates difference between participant and comparison groups reaches significance at the .05 level.

**It should be noted that enrollment here does not necessarily imply full-time student status or that the individual is out of the labor force. In the 24 to 40-month follow-up wave, only 4% of participant males, 2% of comparison males, 6% of participant females, and 5% of comparison females listed education or training as their current major activity.

TABLE V-3

Enrollment in Educational or Training Programs Prior to 24 to 40-Month Follow-up, by Participant Group Status and Sex*

	Participants	Comparisons
Males		
CETA	25%	25%
Technical or trade courses	40%	34%
College courses	10%	6%
Union apprenticeship	3%	3%
Job Corps	4%	8%
Other training	7%	10%
n	179	154
Females		
CETA	26%	29%
Technical or trade	29%	31%
College courses	10%	10%
Union apprenticeship	1%	0
Job Corps	3%	7%
Other training	6%	7%
n	272	238

*This table does not represent "pre-program" educational attainment, but rather education and training already completed at the 24 to 40-month follow-up (as opposed to being still in progress). Since most of the substantive training and education represented in the table occurred since the baseline data was collected, differences between the participant and comparison groups should not be controlled for in the outome analysis in Chapter III (because program participation could have affected subsequent decisions to enroll in the Job Corps, a community college, or a technical school).

TABLE V-4

Current Enrollment in Educational or Training Programs at 24 to 40-Month Follow-up, by Participant Group Status and Sex

	Participants	Comparisons
Males		
CETA	2%	1%
Regular high school	1%	2%
GED program	5%	6%
Technical or trade school	6%	5%
4-year college	1%	Ø
2-year college	3%	1%
College courses	1%	1%
Union apprenticeship	1%	Ø
Other training	1%	1%
n	179	154
Females		
CETA	3%	3%
Regular high school	1%	.4%
GED	7%	4%*
Technical or trade school	7%	5%
4-year college	.4%	.4%
2-year college	2%	1%
College courses	Ø	1%
Union apprenticeship	.4%	Ø
Other training	1%	2%
n	272	238

*Indicates difference between participant and comparison groups reaches statistical significance at the .Ø5 level.

Finally, we examined possible program effects on welfare dependency of participants. In integrating these results, it should be recalled from the previous chapter that a slightly smaller percentage of participant females have children relative to comparisons (63% versus 68%) and that the children of participants will tend to be a little younger on average than the children of comparisons. Table V-5 contrasts participant and comparison group members on a variety of measures of welfare dependency.

Table V-5 indicates no post-program difference in welfare or foodstamp recipiency between participant and comparison females. For males, no clear differences between participants and comparisons emerge, although males are much less likely to receive transfer payments than females.

Summary

The only other benefit of the 70001 program that was found to be statistically significant is attainment of GED certificates: 31 percent of participant males and 21 percent participant females have obtained a GED as opposed to 12 percent of comparison males and 13 percent of comparison females. It is possible that this will have longer term effects on participant labor market outcomes.

We found no evidence that the program has effects on other outcomes such as military status, criminal activity, marriage, enrollment in educational and training programs, or dependency on welfare or parental support.

TABLE V-5

Dependency on AFDC and Foodstamps, by Participant Group Status and Sex

	Females	
	Participants	Comparisons
AFDC at Intake[a]	28%	38%*
Currently receiving AFDC	35%	33%
Currently receiving Foodstamps	38%	34%
AFDC last year	34%	35%
Foodstamps last year	43%	43%
Months on AFDC last year, for those on AFDC	9.8	9.7
Average AFDC per month, for those currently on AFDC	$215.	$202.
n	272	238

	Males	
	Participant	Comparison
AFDC at Intake[a]	7%	27%
Currently receiving AFDC	Ø	1%
Currently receiving Foodstamps	7%	5%
AFDC last year	Ø	1%
Foodstamps last year	7%	11%
n	179	154

*Indicates differences between participant and comparison groups reaches statistical significance at the .Ø5 level.

[a] Note that AFDC at intake included assistance both to the youth and to their families.

CHAPTER VI

PROGRAM COST-EFFECTIVENESS

In the previous P/PV report on pre-employment programs, we found evidence that the labor market effects of the JFY-Boston program were found to have decayed over time. With the follow-up data available at that time, it was unclear whether effects of the 70001 program would be maintained over time or whether they would also decay. The report concluded that follow-up information over a more extended period of time was needed before a judgment could be made on the long-term cost-effectiveness of the 70001 program. With the additional data from the 24 to 40-month interviews, it is now possible to make a more definitive estimation of 70001's cost-effectiveness.

There are three perspectives from which to analyze the costs and benefits of the 70001 program -- society as a whole, taxpayers, and individual participants. From the point of view of the government, the most appropriate benefit-cost analysis is from the perspective of society as a whole. From the societal perspective, 70001's costs include (1) the costs of operating the program at each local site, as well as any administrative costs at the national level; and (2) the opportunity costs to individuals for participating in the program. Benefits from the program, from a societal perspective, include (1) increases in post-program earnings of participants; and (2) other potential benefits from the program such as reduced criminal behavior, drug abuse and reliance on government transfer programs. In the benefit-cost ratios and payback periods estimated below, only post-program earnings gains of participants are included as benefits. No attempt is made to include estimates of reduced criminal behavior since we had not made a concerted effort to obtain before and after measures on such factors. From the cost side, we have excluded information on costs at the national level.

The 70001 program is fairly inexpensive in terms of operational costs. Table VI-1 lays out the costs of operation of the program and calculation of costs per participant. The $1,351 cost per participant figure is derived from program costs in 1979 and the first quarter of 1980, when the youth in the sample entered the program.

Given that 70001 participants only take classroom training for a few weeks, the program is also fairly inexpensive in terms of opportunity costs. Even if half the participants could have found jobs within the first week of classroom training, the opportunity cost would only amount to $100 per participant. The benefit-cost ratios calculated below include $100 per participant in opportunity costs. Because opportunity costs are low relative to administrative costs, the benefit-cost ratios are not particu-

TABLE VI-1

Costs in the 70001 Program
(January 1979-April 1980)

Expenditures

Operating	$626,493
Administration	108,691
Total	$735,184
Number of participants	544
Number of participants placed	296
Cost per participant	$1,351
Cost per placed participant	$2,480

larly sensitive to the opportunity cost figure chosen. Even if zero opportunity costs are assumed -- or if the $100 opportunity cost figure is doubled -- the benefit-cost ratios remain roughly the same.

In the benefit-cost ratios presented in Table VI-2, the benefits are derived from the estimates of program earnings effects over time listed in Table III-9. As alternative estimates of program impact over time were presented in Chapter III depending on various model specifications, alternative benefit-cost ratios are presented here depending on the model specification and discount rate chosen.

In the past report, positive yet statistically insignificant values of program impact were utilized as the estimates of program effects rather than zero, because the program effect is more likely to be that estimate than it is to be zero, even though the estimate is not statistically different from zero. We follow this convention here as well. This means that positive, yet statistically insignificant, values are treated as the program benefit, as are negative and statistically insignificant results.

Last, it should be noted that substitution effects are ignored, therefore potentially overstating the program's cost-effectiveness.

As the results in Table VI-2 indicate, the somewhat crude cost-benefit analysis provides evidence that benefits from the 70001 program outweigh costs for male participants under all model specifications. For females, and for the combined sample, program benefits do not outweigh costs under any model specification. The results for females and the total sample derive from the fact that early participant earnings gains are offset by later losses vis-a-vis the comparison group.

TABLE VI-2

Benefit-Cost Ratios Under Alternative Discount Rates and Modeling Specifications

	Model 1	Model 2	Model 3	Model 4
Males				
5% discount rate	3.19	2.92	1.32	2.20
10% discount rate	3.01	2.79	1.30	2.13
Females				
5% discount rate	-2.15	-1.18	.44	.17
10% discount rate	-1.90	-1.11	.42	.18
Total				
5% discount rate	.29	.49[a]	.95	.99[a]
10% discount rate	.33	.47[a]	.94	.97[a]

NOTES:

Model 1 is wave by wave.
Model 2 is pooled.
Model 3 is wave by wave, with no correction for selection bias.
Model 4 is pooled, with no correction for selection bias.

[a]Since pooled models could not be estimated for the total sample to compute these numbers, these calculations were done by weighting the male and female estimates by their proportion in the sample in each wave.

Table VI-3 extends the benefit-cost analysis to a determination of the program's payback period under alternative model specifications. A pay back analysis asks the question, "How long will it take participants to pay back society for the cost of the program with the increased earnings resulting from their program participation?"

As is shown in Table VI-3, males are able to pay back the cost of the program in slightly over a year. Females are never able to pay back the cost of the program. For the combined sample, because negative effects for females balance positive effects for males, the program is not quite able to pay back its costs under any model specification.

TABLE VI-3

Pay back Period in Years of the 70001 Program
Under Alternative Model Specifications
(Zero Discount Rate)

	Model 1	Model 2	Model 3	Model 4
Males	1.2	1.1	1.3	1.2
Females	xx	xx	xx	xx
Total	xx	xx[a]	xx	xx[a]

NOTES:

xx indicates the program never pays back its costs.
Model 1 is wave by wave.
Model 2 is pooled.
Model 3 is wave by wave, with no correction for selection bias.
Model 4 is pooled, with no correction for selection bias.

[a] Since pooled models could not be estimated for the total sample, these pay back calculations were done by weighting the male and female estimates by their proportion in the sample at each wave.

In the previous P/PV pre-employment report, we concluded that additional follow-up information was needed on the 70001 program before a final determination on program cost-effectiveness could be made. The report also stated that if simple deceleration of program effects over time continued, the 70001 program's payback period would be 1.04 years. The additional follow-up data shows that program effects have decayed much more rapidly than expected, and that for the combined sample, the program is never quite able to pay back its costs.

Summary

Benefit-cost calculations for the 70001 program were done under alternative model specifications and discount rate assumptions. For males, program benefits outweigh costs. For females and for the combined sample, program benefits do not outweigh costs. A pay back analysis also was used to evaluate program cost-effectiveness. For males, the program pays for itself in slightly over a year. For females, as for the combined sample, the program never reaches a point of paying for itself, regardless of the model specification used.

It should be remembered that this cost-effectiveness analysis is a bit crude and pertains to the five 70001 sites as operated from 1979 to early 1980. Changes in the program since then may have altered costs as well as benefit streams.

Notes

1) Because there appear to be stronger program effects for males than for females, at first glance it may appear that the program could increase its long run impact simply by increasing the number of males relative to females in the program. However, there may be stronger effects for males precisely because they are fewer in number in the program. The males who do enroll in the program may self-select themselves because they have needs the program can fill. A more intensive recruitment of males may simply bring in individuals who are not particularly suited to the program.

CHAPTER VII

SUMMARY AND IMPLICATIONS FOR POLICY

This report represents the completion of P/PV's evaluation of pre-employment programs as potential strategies for moving disadvantaged youth into private sector jobs. It has inspected the longer run impacts of the 70001 program by analyzing data from the last wave of follow-up interviews of 70001 participants and comparison group members at 24 to 40 months after intake into the program. In this chapter, we summarize what was learned from the last wave of follow-up interviews and then make recommendations.

WHAT WE HAVE LEARNED

The following findings are derived from the last wave of follow-up interviews on 70001:

- We found no evidence that the 70001 programs investigated here have impacts on the longer run employment and earnings of their participants. While positive effects of the program were found at "9" and "14" months after intake, by the 24 to 40-month post-intake period, we found evidence that these effects had decayed. The effects in the 24 to 40-month post-intake period are near zero for the total sample and for males, but negative and statistically significant for females. No evidence was uncovered to indicate that 70001 impacted on the occupational skill level of participants nor on the annual earnings of participants in calendar year 1981, that is, 21 to 33 months after program intake.

- There is some evidence that the program positively impacts on acquisition of schooling.

- The apparent decay of program effects in this 24 to 40-month period results in large part from an improvement in the labor market position of the comparison group, in the face of a deep recession, at the same time that the participants are experiencing a slight deterioration of their position in the labor market. There is little convincing evidence that the recession was largely responsible for the decay of program effects. Differential childbearing by the participant and comparison groups cannot explain the negative program effects for females, although childbearing and responsibility for dependents has clearly been influential in the differential patterns of labor force behaviors of the male and female participants.

- The program is relatively inexpensive when compared with other treatment modalities. However, it is one of the

more expensive pre-employment programs that have been studied by the Department of Labor.

- When using a crude cost-benefit technique (payback analysis), we found evidence that the program is not cost-effective for females, as females can never pay back the cost of the program. The program appears to be quite cost effective for males as they pay back the cost of the program in just over one year, and does not appear to be cost-effective for the overall sample, as participants come close to but are never quite able to pay back the cost of the program. (Note that this reversal from the earlier report is due to the fact that early participant gains were offset by subsequent participant losses relative to the comparison group.)

This last wave of follow-ups for 70001 was done because certain questions remained after completing the earlier "14 month" post-intake analysis. JFY and 70001, two roughly comparable pre-employment programs, exhibited divergent patterns of effects. While a number of explanations could be ruled out empirically, we could not ascertain whether these divergent patterns were due to differences in the programs or whether the two programs exhibited the same pattern of effects with the 70001 program effects lagged by a number of months due to the use of artifical termination dates. The results from this third wave of analysis help to resolve the quandary.

We now have evidence to support the explanation that 70001 and JFY have comparable patterns of program effects, with 70001 effects being lagged a number of months most probably due to the use of artifical termination dates. Both programs have positive effects in the short-run but program effects decay quite rapidly as comparison youth find their own jobs.

IMPLICATIONS FOR POLICY MAKERS

The research confirms that well-run pre-employment programs such as 70001 can:

- place disadvantaged, out-of-school youth in private sector jobs at a low cost per placement and without offering direct subsidies to employers (nor do employers take advantage of tax credits)
- attract same youth to job-readiness training activities without paying a stipend
- increase youth earnings, at least temporarily, relative to a comparison group
- work for youth under 18 as well as for older youth.

Moreover, it should be noted that the 70001 programs investigated here have a more enduring labor market impact than many other such pre-employment programs studied by the Department of Labor.

However, we have found no evidence that the 70001 programs investigated here have permanent or long run effects on the employability of youth, as measured by subsequent likelihood of employment, earnings or type of job held at 24 to 40 months after intake to the program. (Note, in this longer term follow-up, participation in 70001 did positively affect GED attainment.) Thus, while the program meets many of its objectives, such as placing youth in private sector jobs and increasing earnings in the short run, we find no evidence of the ability of these five 70001 programs to permanently affect the youths' longer run employability. In short, the programs place youth in private sector jobs, allowing them to reap earnings gains, but eventually comparison group members will get jobs on their own and the advantage to participants disappears. Despite the fact that the 70001 programs meet many important objectives, one is left wondering whether pre-employment programs such as 70001 are good social investments. For while these programs are able to increase participants' earnings in the short run, they do not appear to be able to increase youths' earnings in the long run. Additional interventions might be required subsequent to pre-employment experience to sustain the earnings gains produced by the program. Especially for youth under 18 years of age, pre-employment programs like 70001 may be only the first step in a series of necessary interventions.

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APPENDIX A

SUPPLEMENTARY TABLES

TABLE A-II-1

Characteristics at Intake, Participant and Comparison Samples, Male

	Participants	Comparisons
Average age	17.74	17.91
Ethnicity		
black	54%	59%
Hispanic	27%	20%
white	17%	20%
High School degree	1%	1%
Average reading score	6.9	6.7
Average pre-program wage	$2.47	$2.87*
Ever held pre-program job	82%	86%
Average number of dependents	.12	.27*
Head of household	6%	10%
Member of family	80%	74%
Family of one	14%	16%
Receiving AFDC	8%	2%
n	218	176

*Indicates difference between the participant and comparison groups reaches statistical significance at the .05 level.

TABLE A-II-2

Characteristics at Intake, Participant and Comparison Samples, Female

	Participants	Comparisons
Average age	18.03	18.39*
Ethnicity		
black	68%	66%
Hispanic	21%	20%
white	9%	13%
With High School degree	1%	1%
Average reading score	6.7	6.6
Average pre-program wage	$1.79	$1.93
Ever held pre-program job	62%	68%
Average number of dependents	.49	.65*
Head of household	16%	16%
Member of a family	75%	78%
Family of one	9%	6%
Receiving AFDC	22%	24%
n	317	264

*Indicates difference between the participant and comparison groups reaches statistical significance at the .05 level.

TABLE A-II-3

Attrition in 24 to 40-Month Follow-up Sample, Male

	Participants		Comparisons	
	Followed-Up	Attriters	Followed-Up	Attriters
Average age	17.78	17.56	17.82	18.55
Ethnicity				
black	54%	56%	58%	68%
Hispanic	28%	18%	22%	9%
white	16%	21%	20%	23%
With High School degree	1%	0%	1%	0%
Average reading score	7.0	6.6	6.6	7.0
Ever held pre-program job	81%	85%	86%	86%
Average pre-program wage	$2.46**	$2.51	$2.84	$3.03
Average number of dependents	.11**	.18	.23	.50
Head of household	5%	8%	8%	23%*
Member of family	82%	77%	78%	50%
Family of one	13%	15%	14%	27%
Receiving AFDC	8%**	8%	1%	5%
Atlanta	11%	18%	12%	32%
Boston	16%	18%	21%	32%
Richmond	25%	21%	20%	9%
San Antonio	30%**	13%*	19%	0%*
Tulsa	19%	31%	28%	27%
n	179	39	154	22

*Indicates difference between those followed-up and attriters reaches statistical significance at the .05 level.

**Indicates difference between participants and comparisons followed-up reaches statistical significance at the .05 level.

TABLE A-II-4

Attrition in 24 to 40-Month Follow-up Sample, Female

	Participants		Comparisons	
	Followed-Up	Attriters	Followed-Up	Attriters
Average Age	17.99	18.24	18.41	18.23
Ethnicity				
black	69%	58%	67%	58%
Hispanic	21%	20%	21%	19%
white	8%	18%	12%	23%
High School degree	1%	2%	1%	0
Average reading score	6.7	6.8	6.6	7.2
Ever held pre-program job	62%	64%	67%	81%
Average pre-program wage	$1.77	$1.91	$1.87	$2.46*
Average number of dependents	.50	.42	.64	.73
Head of household	17%	13%	17%	0*
Member of family	75%	71%	78%	77%
Family of one	8%	16%	5%	23%
Receiving AFDC	24%	11%*	24%	19%
Atlanta	23%	22%	29%	31%
Boston	20%	27%	9%	15%
Richmond	20%	9%	20%	15%
San Antonio	24%	20%	23%	23%
Tulsa	13%	22%	18%	15%
n	272	45	238	26

*Indicates difference between those followed-up and attriters reaches statistical significance at the .05 level.

**Indicates difference between participants and comparisons followed-up reaches statistical significance at the .05 level.

TABLE A-II-5

Probit Estimates of Selection into 70001 Program, Males

	Coefficient	t - Ratio
Ethnicity		
black	.268	-1.10
Hispanic	-.530	-1.20
Age at intake	-.052	-1.01
Education level	-.584	-6.81*
Head of household	.012	0.03
Family of one	.222	1.00
Number of dependents	-.366	-2.11*
Intake in winter	-.700	-2.54*
Intake in spring	-.511	-2.51*
Intake in summer	.123	-0.58
Local unemployment rate	-.018	-0.12
Ever held pre-program job	.134	0.61
Employed at intake	-1.196	-4.96*
Atlanta (Top Zip)	.271	2.10*
Boston (Top Zip)	.772	2.10*
Richmond (Top Zip)	.091	0.29
San Antonio (Top Zip)	-.282	-0.83
Tulsa (Top Zip)	1.566	3.67*
Atlanta	.683	0.99
Boston	.246	0.46
Richmond	.671	1.89
San Antonio	1.286	1.87
Constant	1.043	1.08

Sample size = 392
Proportion of participants in sample = 55%
Chi-square = 142.00* with 22 degrees of freedom

*Indicates statistical significance at the .05 level.

TABLE A-II-6

Probit Estimates of Selection into 70001 Program, Females

Variables	Coefficient	t - Ratio
Ethnicity		
black	.102	0.45
Hispanic	-.038	-0.12
Age at intake	-.038	-0.91
Education level	-.503	-7.17*
Head of household	.285	1.59
Family of one	.307	1.35
Number of dependents	-.253	-2.63*
Intake in winter	-.117	-0.56
Intake in spring	-.259	-1.56
Intake in summer	.274	1.55
Local unemployment rate	-.052	-0.46
Ever held pre-program job	.231	1.75
Employed at intake	-1.299	-5.45*
Atlanta (Top Zip)	.374	1.71
Boston (Top Zip)	.129	0.42
Richmond (Top Zip)	.436	1.63
San Antonio (Top Zip)	-.178	-0.66
Tulsa (Top Zip)	1.029	2.68*
Atlanta	.469	0.89
Boston	1.062	2.36*
Richmond	.081	0.26
San Antonio	.779	1.61
Constant	2.031	2.52*

Sample size = 579
Proportion of participants in sample = 55%
Chi-square = 166.07* with 22 degrees of freedom

*Indicates statistical significance at the .05 level.

TABLE A-III-1

Estimates of the Effect of the 70001 Program on the Probability of Employment at 24 to 40-Month Follow-up, Total Sample

Independent Variables	Coefficient	t-ratio
Ethnicity		
black	-.49	-3.18*
Hispanic	-.37	-1.50
Education level	-.02	-.32
Ever held pre-program job	.17	1.60
Age at follow-up	-.06	1.76
Dependents at intake	-.09	-1.02
Head of household	-.003	-.02
Family of one	-.02	-.13
Sex		
male	.37	3.67*
Atlanta	-.18	-1.09
Boston	-.17	-1.01
Richmond	-.18	-1.00
San Antonio	.13	.54
Lambda (correction for selection bias)	-.01	-.05
Constant	-1.09	-1.58

Sample size = 828
Proportion employed = .40
Chi-square with 15 degrees of freedom = 62*

*Indicates statistical significance at the .05 level.

TABLE A-III-2

Estimates of the Effect of the 70001 Program on the Probability of Employment at 24 to 40-Month Follow-up, by Sex

Independent Variables	Males Coefficient (t-ratio)	Females Coefficient (t-ratio)
Ethnicity		
black	-.35	-.64
	(-1.62)	(-2.94)*
Hispanic	-.01	-.73
	(-.02)	(-2.33)*
Education level	.04	-.06
	(.34)	(-.69)
Ever held pre-program job	.21	.16
	(1.04)	(1.24)
Age at follow-up	.03	.06
	(.61)	(1.36)
Dependents at intake	.19	-.17
	(.91)	(-1.62)
Head of household	-.21	.01
	(-.53)	(.04)
Family of one	.23	-.16
	(1.02)	(-.62)
Participant	.37	-.40
	(.94)	(-1.07)
Atlanta	.13	-.32
	(.47)	(-1.50)
Boston	.03	-.23
	(.12)	(-.96)
Richmond	-.29	-.09
	(-1.20)	(-.43)
San Antonio	.32	-.00
	(.72)	(-.01)
Lambda (correction for selection bias)	-.23	.14
	(-.88)	(.61)
Constant	-1.00	-.53
	(-.99)	(-.56)
Sample size	324	504
Proportion employed	.51	.33
Chi-square with 14 degrees of freedom	27*	31*

*Indicates statistical significance at the .05 level.

TABLE A-III-3

Adjusted Estimates of Effects of the 70001 Program on Weekly Earnings Total Sample (Without Correction for Selection Bias)

Independent Variables	Coefficient[a]	t-ratio
Ethnicity		
black	-35.69	-6.41*
Hispanic	-41.15	-4.49*
Education level	-1.12	-.56
Ever held a pre-program job	11.42	2.68*
Age at follow-up	.78	.65
Dependents at intake	2.89	.84
Head of household	-7.98	-1.23
Family of one	15.54	2.64*
Sex		
male	43.41	11.08*
Participant	-2.12	-.55
Atlanta	-10.45	-1.65
Boston	-1.55	-.26
Richmond	-22.84	-3.61*
San Antonio	13.90	1.54
Constant	-3.71	-.15
Sigma	39.95	21.85*

Sample size = 828
Proportion of sample with positive earnings (above limit) = .40
Mean weekly earnings = $62.72

[a]Coefficients have been adjusted by the proportion above the limit.

*Indicates statistical significance at the .05 level.

TABLE A-III-4

Adjusted Estimates of the Effect of the 70001 Program on Weekly Earnings at 24 to 40-Month Follow-up, by Sex (Without Correction for Selection Bias)

Independent Variables	Males Coefficient (t-ratio)	Females Coefficient (t-ratio)
Ethnicity: black	-39.84 (-2.02)*	-39.39 (-8.62)*
Hispanic	-51.23 (-.97)	45.52 (-6.52)*
Education level	-4.03 (-.56)	-.47 (-.29)
Ever held pre-program job	28.90 (1.94)	7.73 (2.61)*
Age at follow-up	4.37 (1.07)	.83 (.90)
Dependents at intake	17.94 (.79)	-2.55 (-1.14)
Head of household	-59.79 (-1.49)	-.01 (-.00)
Family of one	29.68 (1.35)	2.21 (.41)
Participant	4.30 (.32)	4.93 (-1.69)
Atlanta	-10.30 (-.45)	-13.07 (-2.78)*
Boston	17.19 (.69)	9.50 (-1.94)
Richmond	-42.19 (-2.37)*	10.66 (-2.17)*
San Antonio	55.10 (1.03)	-.02 (-.00)
Constant	-57.97 (-.72)	22.83 (1.20)
Sigma	95.70	19.26
Sample size	324	504
Proportion with positive earnings (above limit)	.54	.32
Mean weekly earnings	$97.22	$40.54

[a] Coefficients have been adjusted by the proportion above the limit.

* Indicates coefficients which reach statistical significance at the .05 level.

TABLE A-III-5

Occupations at 24 to 40-Month Follow-up, Males

	Participants n=99	Comparisons n=82
Food service workers	18%	16%
Office/clerical	2%	1%
Health care	4%	4%
General labor	37%	40%
Operatives	11%	15%
Crafts workers	19%	15%
Other	9%	10%

TABLE A-III-6

Occupations at 24 to 40-Month Follow-up, Females

	Participants n=80	Comparisons n=81
Food service workers	30%	22%
Office/clerical	20%	17%
Health care/child care	14%	16%
General labor	15%	9%
Operatives	14%	11%
Crafts workers	1%	4%
Other	4%	11%

TABLE A-V-1

Labor Force Status at 24 to 40-month Follow-up
and Reasons Given for Not Being in the Labor Force

	Participants	Comparisons
Males		
Working	54%	49%
Looking for work	31%	37%
Out of the labor force:		
-cannot find work	1%	0%
-lacks necessary schooling or training	2%	0%
-family responsibilities	1%	1%
-in school or training	4%	1%
-ill health	1%	3%
-no transportation	1%	0%
-in jail	4%	6%
-no reason given	1%	3%
	100%	100%
Females		
Working	29%	37%
Looking for work	39%	37%
Out of the labor force		
-believes no work available	1%	1%
-cannot find work	1%	1%
-lacks necessary schooling or training	5%	3%
-cannot arrange child care	7%	5%
-family responsibility	10%	6%
-in school or training	4%	2%
-ill health	1%	2%
-pregnant	6%	6%
-no transportation	1%	1%
-in jail	1%	0%
	100%	100%

APPENDIX B

POOLING VS. WAVE-BY-WAVE ESTIMATION

In determining changes in program impacts over time, there is some question whether program effects should be estimated using a pooled data set of all three follow-up waves or using each of the three follow-up waves individually. Recent evaluations of youth employment programs done for the Department of Labor have taken alternative approaches. The follow-up evaluation of the Job Corps, for example, used pooled data. The follow-up evaluation of the impact of supported work on disadvantaged youth used unpooled, wave-by-wave data.

Using pooled data increases the sample size and thus lowers the standard errors of estimates. The use of pooled data, however, restricts the coefficients of independent variables to be the same across time periods. The use of pooled data also means that the observations should appropriately be weighted by an error-components technique to remove possible correlation in the variances of the error terms of different observations on the same individual. This error-components method is straightforward when used with ordinary least squares estimation. In this evaluation, in which the dependent variables for the most part require either a maximum likelihood or method of moments estimation procedure, the error-components technique presents more of a problem. If pooled observations are not weighted, the resulting estimates of program effects are still unbiased, but the standard errors may be inappropriately low.

To take into account possible differences in estimated effects using pooled and unpooled data, we presented the results in Chapter III using both pooled and wave-by-wave data. In the wave-by-wave analysis, the 7 to 12 month results come entirely from the "9"-month follow-up wave, the 13 to 18 and 19 to 24 month results come from the "14"-month follow-up wave and the 24 to 29, 30 to 35, and 36 to 40 month results come from the 24 to 40-month follow-up wave.

As is clear in the summary table at the end of Chapter III the results are very similar whether pooled or wave-by-wave data are used. The pooled estimates do have lower standard errors. In part, this is due to the larger sample sizes in the pooled data set and in part due to the fact that the standard errors may be inappropriately low because the observations have been left unweighted.